FISCAL YEAR 1985 DEPARTMENT OF ENERGY AUTHORIZATION

(Magnetic Fusion Energy)

HEARINGS

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND PRODUCTION

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

NINETY-EIGHTH CONGRESS

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1985 DEPARTMENT OF ENERGY AUTHORIZATION

THURSDAY, FEBRUARY 23, 1984

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Research and Production,
Washington, D.C.

The subcommittee met, pursuant to notice, at 2 p.m., in room 2318, Rayburn House Office Building, Hon. Stan Lundine presiding.

Present: Representatives Lundine, Lowery, and Chandler.

Staff present: Dr. John V. Dugan, staff director; Ron Williams, technical consultant; Bill Harvey, minority energy coordinator; and Dave Clement, minority counsel.

Mr. Lundine [presiding]. Good afternoon, ladies and gentlemen. The subcommittee believes that the challenge for the magnetic fusion energy program is to sustain the momentum of the program and the very critical industrial involvement. There is a broad consensus on what the next U.S. fusion device should be; that is, a Tokamak facility in which the plasma can be ignited and burned and which is also capable of long-pulse operation.

The fusion community, the DOE's Magnetic Fusion Advisory Committee, and the Energy Research Advisory Board, ERAB, panel all agree on this fact.

The subcommittee's advisory panel took the position last year, that in order to maintain international preeminence, the United States must move ahead on the next device—the Tokamak fusion core experiment, or TFCX—which would combine significant scientific and engineering thrusts.

An issue which remains to be resolved during fiscal year 1985 is the extent to which the new facility should be capable of a subsequent upgrade. It is also an open question as to whether DOE might not opt for a lower-cost version of TFCX which would still meet the ignition and long-pulse requirements.

The stakes are clear, U.S. international preeminence and ultimately the market position in this technology depends on sustaining program momentum. As the DOE MFAC has recommended, however, this initiative should not require international arrangements as a prerequisite, a prerequisite for going ahead.

The United States must stay in the driver's seat, and we now have no choice but to move ahead. In this manner, we can arrive at mutually acceptable arrangements for cooperation with Japan and/or other technology partners who wish to remain at the forefront of fusion technology.

Dr. Trivelpiece and Dr. Clarke, welcome. Please proceed with your testimony.

STATEMENT OF DR. ALVIN T. TRIVELPIECE, DIRECTOR, OFFICE OF ENERGY RESEARCH, U.S. DEPARTMENT OF ENERGY, ACCOMPANIED BY DR. JOHN F. CLARKE, ASSOCIATE DIRECTOR FOR FUSION ENERGY, OFFICE OF ENERGY RESEARCH, U.S. DEPARTMENT OF ENERGY

Dr. **Trivelpiece**. Mr. Chairman, it's a pleasure to be here today, certainly.

Mr. **Lundine**. Excuse me. I apologize for interrupting. Without objection, we will enter opening statements into the record by Mr. Winn and Mr. Walker at this time.

[The prepared opening remarks of Mr. Winn and Mr. Walker follows:]

STATEMENT BY THE HONORABLE LARRY WINN, JR.
ENERGY RESEARCH AND PRODUCTION SUBCOMMITTEE
HEARING ON MAGNETIC FUSION
FEBRUARY 23, 1984

THANK YOU, MADAM CHAIRMAN. ONCE AGAIN WE WELCOME DR. TRIVELPIECE TO OUR HEARING. THIS MAKES HIS THIRD APPEARANCE IN TWO DAYS FOR COMMITTEE BUDGET HEARINGS, AND HIS PRESENTATIONS AND RESPONSES ARE GREATLY APPRECIATED.

THE MAGNETIC FUSION PROGRAM, WHICH WE WILL DISCUSS THIS AFTERNOON, REPRESENTS ONE OF THOSE TECHNOLOGIES FOR WHICH FEDERAL FUNDING IS VERY APPROPRIATE. IN TESTIMONY RECEIVED FROM WITNESSES LAST YEAR WE WERE ADVISED THAT COMMERCIAL APPLICATION OF MAGNETIC FUSION PROBABLY WILL TAKE PLACE SOME 50 YEARS FROM NOW. A SUBSTANTIAL EFFORT AND MANY GRATIFYING RESULTS HAVE BEEN OBTAINED TO DATE, BUT WE ARE ALL AWARE THAT MANY CHALLENGES MUST BE MET AND OVERCOME IN THE FUTURE. SUCH HIGH-RISK, LONG-TERM PROGRAMS ARE PROPERLY IN THE REALM OF GOVERNMENT FUNDING. WE WOULD CERTAINLY EXPECT INDUSTRIAL PARTICIPATION AS WE GOT CLOSER TO DEMONSTRATION OF THE TECHNOLOGY, BUT AT THIS TIME FEDERAL SPONSORSHIP IS FITTING AND PROPER.

THE COMMITTEE HAS ALSO RECEIVED TESTIMONY IN YEARS PAST FROM COLLEGES AND UNIVERSITIES AND FROM INDUSTRY THAT THE EXTENT OF THEIR PARTICIPATION IN THE MAGNETIC FUSION PROGRAM IS VERY SMALL.

THEY ALL BELIEVED THAT THEIR INCREASED PARTICIPATION WOULD BE BENEFICIAL TO EVERYONE CONCERNED WITH THE PROGRAM. I AM CERTAIN THAT THE DEPARTMENT OF ENERGY HEARS THESE SAME COMMENTS AND WE MENTION THEM HERE ON THE BASIS THAT THE COMMITTEE IS INTERESTED.

THE AMOUNT OF MONEY PROVIDED TO THE MAGNETIC FUSION PROGRAM IS QUITE LARGE. THE HALF BILLION DOLLAR PER YEAR LEVEL IS TREMENDOUS WHEN COMPARED TO MANY OTHER PROGRAMS. I REALIZE THAT THE PURPOSE IS TO COMPARE TWO BASIC CONCEPTS, THE TOKAMAK AND THE MIRROR APPROACHES. HOWEVER, THE TOKAMAK PROCESS IS SOMEWHAT ADVANCED AND IT MAY BE DIFFICULT FOR DOE TO JUSTIFY FULL SCALE FUNDING FOR BOTH APPROACHES. I WOULD CERTAINLY BE GLAD TO GET COMMENTS ON THIS OBSERVATION.

ONCE AGAIN, MADAM CHAIRMAN, WE WELCOME DR. TRIVELPIECE AND I BELIEVE HIS TESTIMONY WILL BE VERY HELPFUL TO US ALL.

THANK YOU.

OPENING REMARKS OF
REPRESENTATIVE ROBERT S. WALKER
FEBRUARY 23, 1984

GOOD AFTERNOON, MR. CHAIRMAN, I WOULD LIKE TO JOIN YOU IN WELCOMING THE WITNESSES HERE BEFORE US TODAY. OUR TOPIC THIS AFTERNOON, MAGNETIC FUSION, IS ONE OF GREAT INTEREST.

THROUGHOUT HISTORY MAN HAS SEARCHED FOR LIMITLESS ENERGY SOURCES, AND IT APPEARS THAT WE MAY BE APPROACHING THE DAY WHEN THIS PROGRAM WILL LEAD US TO REALIZING THAT DREAM.

THE TECHNOLOGY WE ARE DEVELOPING IN THE MAGNETIC FUSION PROGRAM MAY BE READY FOR DEPLOYMENT AS EARLY AS THE TURN OF THE CENTURY. WE HAVE ALREADY MADE QUANTUM LEAPS IN OUR UNDERSTANDING AND OUR ABILITY TO IMPROVE THE QUALITY OF THE CONFINEMENT FROM THE EARLY TOKOMAKS TO THE PROGRAM OF TODAY.

WE HOPE THAT WITHIN TEN YEARS WE WILL ACHIEVE IGNITION WITH THE TFCX AND REACH OUR GOAL OF FUSION. I AM PARTICULARLY INTERESTED IN THE INDICATIONS I HAVE RECEIVED THAT THERE IS NOW GENERAL AGREEMENT THAT WE SHOULD MOVE FORWARD WITH THE TFCX AND THAT THE TANDEM MIRROR CONCEPT WILL BE MOVED TO THE BACK BURNER. I LOOK FORWARD TO THE TESTIMONY.

Now please proceed.

Dr. TRIVELPIECE. Fine. I wanted to first ask that my prepared statement be submitted for the record.

Mr. LUNDINE. Without objection, the entire statement will be made a part of the record.

Dr. TRIVELPIECE. Before turning to some of the specific issues, I would like to make some general remarks. First I would like to comment that I think your opening statement, I think, is very good. I will touch on some of those points in my general remarks, not directly answering them but covering them.

Each year it's been kind of traditional for whoever is testifying on this program to appear before this committee and have a slide show. A slide show includes 15 or 20 viewgraphs, and these viewgraphs start showing how a deutenium ion and a tritium ion collide with one another and release fusion energy and that the particles that are associated with that are—have high speed and that they're charged and that they form a plasma and you need a magnetic field to hold onto them. And quite a few of these diagrams are done.

Most of the members of this committee have been treated to this slide show on one or more occasions. If there are any members who have not and would like to have that as a private briefing, I would be glad to do it. But I would propose to dispense with it today.

The committee is well aware, I think, of most of the advantages of fusion and so on. Those have been submitted for the record in past hearings.

The first thing I wanted to mention is that the budget is $483 million. That's about a 3-percent increase from last year. That is slightly less than the cost-of-living, although it was intended to be a constant level of effort of support by the department and by the administration. And I am sure that many of the scientists working in the field don't particularly agree with this, but from the administration's point of view, it reflects a strong degree of support for the program.

In order to understand that, it's perhaps appropriate to identify the climate in which that statement is made. And the climate that exists and with which I have to contend with and deal at the moment is one in which the supply of oil is going up, the price of oil is going down. Yellowcake has gone from $80 a pound to $20 a pound, the nuclear fuel. There's certainly a substantial deficit which all of us are trying to struggle with and the like. And that in that climate it's difficult to make a case for a rational basis for accelerating the fusion program.

However, there is also no basis for requesting or recommending any reduction in the effort that the collection of individuals who have been brought together in the United States. And the collection of activities that they're doing are clearly of interest to the United States to continue strong support for them.

I would like to talk about this just a moment in the context of Secretary Hodel's remarks which have come from the National Energy Policy Plan Four. And in that the administration desires to minimize, to whatever extent possible, Federal control in certain areas of the energy activities, and that what we're looking for is, as you said, an assured supply, adequate supply at a reasonable cost, and that in doing so, that this should be—there should be a proper balanced mix of the various energy resource and supplies.

Fusion clearly fits into that as one of the administration's programs in which it seeks to have a long-range energy option available to it for fusion. But we're doing this at what we feel is an appropriate and deliberate pace. And that deliberate pace really is based on the fact that the fusion program has been remarkably successful. If you look out at it over the period from 1952 until now, there really has been very systematic, regular progress. There's no setback, and by anybody's standards, is a very successful program.

I want to just back up briefly here and go back to 1973, when my friend, Dr. Bob Hirsch, was director of the fusion program, and he got several of us together in Florida to talk about where the fusion program might go. And he said that if the fusion program is really to be serious and successful, at some point it's going to have to face up to the issue of trying to produce energy. And in that, there was established the idea of DT burning; that is, that somewhere in the fusion program's future at that time, in 1973, a device should be conceived of and brought into existence that had the possibility of actually going at DT burning and not continue to use hydrogen plasmas to explore the physics of plasma but rather to get at plasmas that produce substantial quantities of energy; that there is a connection between that statement of principle—namely, that DT burning should be the next goal of the fusion program—and the TFTR device which is now operational at Princeton. And it was dedicated just this last year by Secretary Hodel at the ceremony at Princeton.

It has been operating very well this last year. It has now achieved confinement times of in excess of three hundred milliseconds. And I think the progress that has been made, with it by scientific standards against which such things are judged is better than had been anticipated by those who designed it and who were operating it. And that's quite—quite remarkable.

The plan is to use this device to address this break-even question in 1986 of DT burning. At that point it is planned to introduce deuterium and tritium into the experiment and perform something like ten shots with it. And 10 shots in a device of this size, which has cost something in excess of $300 million, may not seem like a lot. On the other hand, if it is properly instrumented and done properly, it should provide the kind of information that once and for all lays to rest this issue of can you put together an ionized gas of deuterium and tritium and cause it to be heated to a temperature such that it will release more energy than was invested in putting it together.

So we look forward to that as a result of that time, that the so-called break-even issue should—should be solved.

Well, to continue then, the—fusion, as a scientific exercise, is characterized by two parameters. One is, how hot do you have to get the plasma in order to achieve your goal? And that temperature is thought to be somewhere in the neighborhood of one hundred million degrees.

The Princeton large Torus device a few years ago demonstrated that that temperature could be achieved in a reactor-type plasma.

There is another parameter which is used by scientists who work in this field to characterize the quality of the plasma which they're confining, and it's characterized by the letters N tau, and that's a confinement quality for energy parameter. And it reflects how long you hang on to a plasma at a particular density to produce the energy break-even that is needed.

The Alcator device at MIT this last year achieved a level of 8 times 10 to the 13th. The point is that both of these numbers are above the threshold independently of that which is needed in order to achieve a proper fusion reaction.

The trick is to get them—to get that to occur in the same experiment at the same time. And of course, being above that threshold in one device is what is anticipated for in 1986 in the TFTR device at Princeton.

Before turning to some other things, I want to just mention that the magnetic fusion test facility at Livermore, which is another type of plasma confinement, so-called open confinement in a magnetic mirror, that it's on schedule, that something of 246 million has been spent and that the final increment of 20 million of construction is requested for—in this budget.

In the meantime, while U.S. scientists and engineers have been very successful in the fusion program, the Europeans haven't been asleep either, that there is a device called the Joint European Torus, and this has been operated at a facility near the Cullen Laboratory in England. And they have achieved a level of 3 million amperes in the plasma heating and confinement, quite a remarkable step.

But what—just what is this next step? And you referred to it in your opening remarks? And it is certainly true that our Energy Research Advisory Board, which has had a committee devoted to fusion as well as the Magnetic Fusion Advisory Committee, which I empaneled a few years ago, 1½ years ago, has addressed this question. And much as Hirch in 1973 talked about DT burning, what they're, in effect, saying is that the next goal of the fusion program should be ignition, that it is the remaining scientific question that needs to be addressed.

And what's involved here is that it's roughly like taking a blowtorch and shining it on a log: you can keep the log burning as long as the blowtorch is going, and you're clearly releasing energy; but if you turn the blowtorch off, the log goes out. That's what the present experiment in TFTR, in effect, does. You have to keep the heat applied to it in order to keep the reaction going. On the other hand, if you can turn the blowtorch off and the two-or-three-log combination will continue burning, then you clearly have ignition.

The fusion process involves the release of energy in such a way that some of the particles that are products of that reaction are not contained in the device, TFTR, or other devices, as presently operated, and some increase in size and some increase in magnetic field is needed in order to contain that energy and achieve this ignition process.

So we think that is the next major goal. The trouble is that it's likely to be an expensive step, and it is in this issue of how much it is that I think it makes it incumbent on—on us collectively to make sure we know what it is that we're talking about and have a proper conceptual design. In that regard, we have, within the normal amount of budget that's available for reactor considerations, set aside about $10.8 million in the fiscal year '85 request for design of the TFCX project—TFCX, Tokamak fusion core experiment—which is this ignition device.

Now, exactly what it is going to cost and exactly what it's going to do are things that need to be carefully planned for and thought through. And we're not seeking a line item for it at this—this time.

I think when you get into the issue of how much and where and who is going to do the work, that things will become a bit more lively in terms of the debate. And it's here where I want to address the issue which you also mentioned; that is, international collaboration and cooperation; that fusion, the fusion program, the U.S. fusion program, has really been outstanding as an example of international cooperation and collaboration; that there are very few scientific programs that achieve the same level of cooperation which this program does; that the Doublet device, which is a Tokomak that's operated at the General Atomic, GA Technologies, in San Diego, has had something in excess of $70 million contributed to it both for its operation and equipment in the last 5 years. And then we recently included an agreement for the next 5 years in which Japan will join us in doing research on this particular device.

So in a very practical sense, international collaboration really has worked.

But there is a sort of a qualitative change that takes place, I think, when you begin to talk about devices that exceed several billions of dollars, that it's very clear that the United States has the resources to carry out a program that involves objects that it costs in excess of $1 billion. But the question is, is it really in our interest to do so, and what are the various issues that come to—come to bear on such a consideration?

The—a remarkable event has occurred in the last couple of years, and this event involves the fact that our President, together with the heads of state of six other nations, met first in Versailles, and recognized for the first time that collectively speaking, science and technology are essential ingredients to our economic growth and well-being and that we need to have a strong science and technology component to our individual activities.

And there were several areas which were—the heads of state acknowledged that there should be working groups set up and exploration of international cooperation. Fusion was one of these. The United States shares the lead for this activity with the European Communities. And I have been representing the United States in that activity. And I and some of my counterparts from the other Economic Summit countries met this last year to look into this. And we have now prepared an interim report, and that will serve as the basis for some recommendations and suggestions to be made when the heads of state meet in London this forthcoming June.

I think the kind of issue that they're looking at is—and that we have been discussing with our counterparts—is, given the fact that these are very expensive devices, does it really make sense for the United States and several other countries to simultaneously build precisely the same object with precisely the same object with precisely the same objectives, to come into completion and operation at about the same time, seeking to produce essentially the same result. It seems like a poor use of international resources, or poor use of resources on any basis.

The question is, can we meet and come to some agreement as to what the collection of deliverable items are that need to be done for fusion? Can we, say, look at—there needs to be three things done, and these three things are as follows—without regard to where they are done, what are the three things that are needed? Given that you can come to that, can you then come to an agreement where there would be a mutual relinquishment of some sovereignty on the part of the two participants such that country A does thing one and country B does thing two, and you agree to be bound by completion and exchange of information and so on?

If that could work, it would certainly be a new step and a qualitative change in how international cooperation takes place.

I think this takes some encouragement from heads of state down as distinct from the scientific community up, because I don't believe it would occur on this basis. But as you said, it is important for the United States to maintain leadership in this field, but at the same time, there are other considerations that go into issues of such international collaboration that go beyond the simple scientific merits of it. And those will be explored in conjunction with the London—London Summit, perhaps.

Before turning to questions, I just want to comment that I think the fusion program also has reasonable industrial cooperation and the university communities, that last year we had the Magnetic Fusion Advisory Committee address the university utilization question. And I am pleased to say that they said that we are utilizing the university community in a manner that is consistent with what they can do, and we don't see that there is any problems there. So I am pleased with that.

With regard to industry, there is still the Magnetic Fusion Advisory Committee panel that's exploring that question, and we look forward to their comments and advice in the future.

Mr. Chairman, thank you. That concludes my opening remarks. I would be happy to answer any questions. And Dr. Clarke is here to help as well to answer any questions of a technical nature.

[The prepared statement of Dr. Trivelpiece follows:]

Statement of Alvin W. Trivelpiece

Director of Energy Research

Department of Energy

before the

Subcommittee on Energy Research and Production

of the

House Science and Technology Committee

February 23, 1984

Madam Chairman and Members of the Subcommittee:

It is a pleasure to appear here today to present the FY 1985 budget request for the Magnetic Fusion Energy (MFE) program. This budget request reflects the goals and strategies set out in the Fourth National Energy Policy Plan (NEPP-IV), published in October 1983. As stated in NEPP-IV, the goal of our national energy policy is to foster an adequate supply of energy at reasonable costs. The strategies for achieving this goal are: (1) to minimize Federal control and involvement in energy markets while maintaining public health and safety and environmental quality, and (2) to promote a balanced and mixed energy resource system. In pursuing these strategies, NEPP-IV notes the importance of Federal energy R&D programs aimed at developing new and alternative energy resources by providing scientific and engineering knowledge for further development by the private sector. The MFE program is developing the data base that will make it possible to determine what role fusion could play in mixed energy resource system envisioned by NEPP-IV.

As a long-term energy option, magnetic fusion could become a principal energy source in the next century. Its attractive potential is based upon such characteristics as an inexpensive, vast and secure fuel reserve, environmental and safety aspects that appear to be acceptable, and promising results from scientific research in the U.S. and abroad. Fusion development also contributes important technology advances that provide near-term benefits in fields as diverse as superconducting magnets, high power accelerators and radiofrequency systems, advanced materials development, and mathematical methods for computers.

The goal of Federally-supported, long-term R&D is to develop promising technological concepts to the point where private enterprise can reasonably assess their risks. Accordingly, the Department's Magnetic Fusion Energy program is developing the technical data base required for the private sector to determine the commercial potential of fusion. The program strategy is based on the commitment to develop these data, and to do so in such a way as to maintain an international leadership role for the U.S. in fusion research.

In support of the technical data base goal, the program's intermediate objectives are the development and operation of facilities in which fusion research is conducted, the definition of an attractive reactor confinement concept and the development of critical technologies required to demonstrate engineering feasibility. This work is being done through a base program of research in magnetically confined plasmas, including tokamaks, tandem mirrors, and other supporting concepts. The objective is to select a suitable fusion plasma core for an eventual fusion reactor. This base program also develops the hardware, materials and systems required to complete the scientific program and initiate the complementary reactor engineering development program.

The two leading confinement concepts being pursued are a closed magnetic confinement system referred to as a toroidal system and an open system referred to as a mirror system. The toroidal system uses doughnut shaped devices wrapped with electrical coils that induce a closed magnetic field.

There are several kinds of toroidal devices including tokamaks, stellarators, reversed field pinch systems, and compact toroids. Tokamaks are the devices being examined most extensively both in the U.S. and internationally. The majority of the tokamak research is conducted at the Princeton Plasma Physics Laboratory (PPPL), GA Technologies, Oak Ridge National Laboratory (ORNL), and the Massachusetts Institute of Technology (MIT). The second leading concept is an open magnetic confinement system referred to as a mirror system. Mirror devices have cylindrical center sections with large magnets at each end that reflect the plasma particles toward the center to prevent their escape from the open ends. The major laboratory for mirror research is the Lawrence Livermore National Laboratory (LLNL). Additional mirror experiments are conducted at MIT, the University of Wisconsin, and TRW.

Presently the tokamak is the most developed confinement concept followed by the more recently developed tandem mirror. The tokamak program will be ready by late 1986 to attempt production of reactor grade plasmas using deuterium/tritium fuels at the Tokamak Fusion Test Reactor (TFTR), which recently began operation at Princeton. The Mirror Fusion Test Facility (MFTF-B), which is under construction at Livermore, will investigate by the late 1980's the physics of a reactor-grade mirror plasma using deuterium as a fuel.

There has been general agreement among all parties (i.e., the fusion scientific community and advisory groups) that a next logical program step, generally designated as the Tokamak Fusion Core Experiment (TFCX), should produce a near reactor grade ignited plasma in a long pulse device. There is further agreement that sufficient technical information and program capability now exist to design a next device that will produce an ignited, long pulse plasma. Systems studies of various TFCX options are now underway, with the Princeton Plasma Physics Laboratory and the Fusion Engineering Design Center playing the major roles.

During FY 1983-84, fusion research on the leading concepts and on technology development led to many technical accomplishments. In tokamak research:

o The most outstanding tokamak experimental achievement in 1983 was the attainment of a value for the confinement parameter (defined as the product of density and energy confinement time) above the minimum value required for proof of fusion's scientific feasibility. This result was obtained in the Alcator C confinement device at MIT at densities several times greater than those expected in a fusion reactor.

o The TFTR device, which is designed to achieve energy breakeven, thus proving fusion's scientific feasibility, began experimentation for the first time in FY 1983. Initial experimental results have been extremely encouraging with world record energy containment times of nearly 300 milliseconds.

- The Princeton Large Torus at PPPL demonstrated that the tokamak plasma current can be started and maintained using radiofrequency waves. Although the current drive efficiency in these groundbreaking experiments, when extrapolated to reactor densities, was a little too low for true steady state reactor use, these results provide an expanded experimental foundation for designing tokamak reactors with sufficiently long pulse lengths that loss of strength in construction materials due to cyclic fatigue is not a problem.

In the mirror confinement concept:

- The existence of a thermal barrier was confirmed for the first time in the Tandem Mirror Experiment Upgrade at Lawrence Livermore National Laboratory. This long-sought achievement greatly improves the potential economics of mirror reactors.

- A major accomplishment at the Tandem Mirror Experiment (TMX) device at LLNL showed the technical feasibility of the "Yin-Yang" concept which involves two "C" shaped magnets to produce a magnetic mirror. The TMX tests were carried out successfully without excessive particle escape or the plasma instabilities that could lead to reactor instabilities. Also, for the first time, TMX startup was achieved using only radiofrequency plasma heating, the startup technique most likely to be used in a reactor.

In the area of technology development, progress was made with the demonstration of the efficient production and release of tritium in a solid tritium breeding experiment. Such solid breeders would improve the safety of fusion systems. The processing of tritium is being studied in the Tritium Systems Test Assembly. Other accomplishments included the completion of a test to show how large amounts of lithium can be handled safely, the setting of several world records in the development of components needed to heat fusion plasmas (i.e., neutral beam injectors, gyrotron tubes, pellet injectors), and continued progress in developing component hardware that will be needed for fusion experiments. The Large Coil Test Facility (LCTF) at Oak Ridge National Laboratory and three of the six large superconducting magnets (coils) for use in the facility were completed. Two of the 40 ton coils were installed in the LCTF vacuum tank in preparation for an initial two coil test in early 1984.

During the last year, international cooperation continued to play an important role in the United States' fusion program. Areas of cooperation included sharing research data, joint planning of comprehensive research programs in selected areas, and joint funding and hardware support for major scientific and technological projects. The European Community and Japan are major participants in these activities. Active scientific information exchanges also are continuing with researchers in the Soviet Union.

In September 1983 senior government officials responsible for fusion policy matters participated in the Versailles Summit of Industrial Nations to consider strengthening joint planning and collaboration in fusion energy research. Bilateral activities also were pursued in 1983 and included the signing of an agreement for a joint effort by the U.S. and Japan in the area of materials irradiation activities. Experimental time on the High Flux Isotope Reactor and use of the Oak Ridge Reactor facilities at ORNL will be involved in this research. In addition, DOE and the Japanese Atomic Energy Research Institute extended for five years their joint project in tokamak research on the Doublet III facility at GA Technologies.

Finally, an agreement of cooperation between the U.S. and the Peoples Republic of China (PRC) was signed in 1983.

The strategy used to implement the ongoing MFE program has three phases: scientific feasibility, product definition and product development. Scientific feasibility is defined as a determination that a magnetically confined high temperature plasma ultimately could be used to produce significant amounts of potentially useful energy. The current plan is to attempt to demonstrate scientific feasibility in the TFTR in late 1986. This will occur when energy breakeven is attained (i.e., when the amount of energy released from fusion reactions equals the amount of energy required to heat the plasma).

As confidence increases in our ability to accomplish the objective of the first phase, the program has begun to address the issues of the product definition phase. The objective in this second phase is to identify a reactor core plasma confinement concept that is credible in both a scientific and technological sense. There are two major thrusts in the program to achieve this objective. On one hand, we must confront the issues of burning plasmas in a TFCX-like facility. On the other hand, we must strengthen the search for innovative magnetic configurations that have improved potential for practical application.

FY 1985 Budget Request

The FY 1985 budget request for Magnetic Fusion Energy is $483.1 million. This represents an increase of $12.3 million over the FY 1984 level. The request includes $417.2 million for Operating Expenses, $33.4 million for Capital Equipment funds and $32.5 million for Construction projects (Table 1).

At current funding levels, the program will continue physics research aimed at developing the two most advanced concepts, tokamaks and tandem mirrors, as well as a technology development program to support the base scientific program. In addition, support is provided for developing alternate concepts which contribute to the general base of technical knowledge underlying the main concepts. The requested level of funding will allow us to address the key physics issues related to heating, confining, and controlling a plasma as well as the reactor technology and design issues

Table 1
Magnetic Fusion Energy
Budget Authority
($ in millions)

	FY 1984 Estimate	FY 1985 Request
Operating Expenses	$396.5	$417.2
Capital Equipment	37.8	33.4
Construction	36.5	32.5
Total	$470.8	$483.1

associated with a long pulse ignited tokamak. The technology issues include tasks in the areas of plasma production, reactor components, and system designs. Existing facilities will be used to pursue key technical issues of confinement physics and engineering development. Together, these physics and technology research efforts will provide a base program moving toward the objective of developing a commercially attractive reactor concept.

The Magnetic Fusion Energy budget is organized into five interrelated functional areas that form the basis of a unified and balanced effort to conduct the research and development necessary to define a practical fusion product. The Confinement Systems and the Applied Plasma Physics subprograms, working in conjunction, provide the physics base for the MFE program. In the near term, the primary driving force of that program flows from the experiments conducted in the Confinement Systems subprogram, with its dual focus on toroidal confinement systems and mirror confinement systems. The Applied Plasma Physics (APP) subprogram analyzes these results, develops plasma physics theories and models to explain the experimental results and suggests directions for additional investigations. Based on this information, the APP subprogram also initiates experimental and theoretical investigations of alternate confinement concepts on basic physics problems and develops diagnostic instruments for use by the Confinement Systems subprogram.

Using information developed by the above subprogram, the Development and Technology subprogram develops components and systems for use in near- and mid-term fusion experiments as well as long-term generic materials development. This subprogram also performs conceptual design studies for fusion power reactor concepts. These design studies serve to identify component and system development needs that must be undertaken to prepare for the eventual commercialization of fusion.

In carrying out the assignments of the physics and technology tasks, needs for major new facilities are identified from time to time. The Planning and Projects subprogram provides for construction of major new facilities and fabrication of auxiliary components for these facilities. Finally, the Program Direction subprogram includes the funding to cover salaries, benefits, travel, and associated expenses of the Headquarters Office of Fusion Energy. The proposed budget levels for these areas are indicated in Table 2.

In Confinement Systems, Operating Expenses funds of $154.1 million are provided to support toroidal systems and $42.1 million to support mirror systems. A major portion of the toroidal systems budget provides for the cost of conducting experimental operations on existing devices including: Alcator C at MIT, the Princeton Large Torus/Poloidal Bean Experiment at Princeton, and Doublet III at GA Technologies. In addition, funds will be

Table 2
Magnetic Fusion Energy
Budget Authority
($ in millions)

	FY 1984 Estimate	FY 1985 Request
Operating Expenses		
Confinement Systems		
Toroidal Confinement Systems	$144.2	$154.1
Mirror Confinement Systems	43.9	42.1
Applied Plasma Physics	76.8	84.5
Development & Technology	78.3	79.6
Planning and Projects	48.7	52.8
Program Direction	4.6	4.1
Subtotal	$396.5	$417.2
Capital Equipment	37.8	33.4
Construction		
Fusion Materials Irradiation Test Facility	0.0	3.0
Mirror Fusion Test Facility-B	30.0	20.1
General Plant Projects	6.5	9.4
Subtotal	$ 36.5	$ 32.5
Total	$470.8	$483.1

used to prepare the TFTR for the world's first demonstration of magnetic fusion scientific feasibility by achieving energy breakeven in 1986. Fabrication of the Advance Toroidal Facility at ORNL and modification of the Doublet III facility in cooperation with the Japanese to enhance the capability of this machine will continue in FY 1985.

In mirror confinement systems, the funds will be used to support the operation of the Tandem Mirror Experiment-Upgrade at LLNL, the first full year of operation on TARA at MIT and several other smaller devices to extend the technical data base of tandem mirrors. Preparation for the operation of MFTF-B will also be initiated in FY 1985. Mirror confinement systems also includes support for the Elmo Bumpy Torus (EBT) concept which involves toroidally linked mirrors. Work on EBT will be directed toward continued studies of fundamental issues of physics (e.g., electron cyclotron heating and confinement) in support of the tokamak and tandem mirror programs.

The Operating Expenses budget request for the Applied Plasma Physics subprogram is $84.5 million. This includes support for fusion theory, experimental research on smaller scale tokamaks and mirrors, development of new diagnostic techniques required to understand experimental behavior, development of supporting concepts with potentially superior reactor prospects and operation of the Magnetic Fusion Energy Computer Network which provides all the large scale computing capability used by the fusion program for data analysis, theoretical modeling and design.

The Development and Technology subprogram will be supported in FY 1985 at a level of $79.6 million in Operating Expenses. This will include work on necessary plasma and reactor technologies as well as design of reactor systems. A conceptual design for a long pulse ignited tokamak will be carried out by redirecting efforts from within the program. Specific technology development issues include plasma heating; plasma fueling; impurity and exhaust control; first wall, structure and heat flux materials; blanket and shield systems; superconducting magnets; and remote maintenance. The major facilities for technology development which will be funded in FY 1985 include the Large Coil Test Facility and Rotating Target Neutron Source II Facility at ORNL and the Tritium Systems Test Assembly at LANL.

The Planning and Projects subprogram Operating Expenses request is $52.8 million. This includes completion of work on the Tokamak Flexibility Modification project for TFTR to ensure achievement of the basic TFTR objectives. An increase is provided for development support and fabrication of auxiliary components on MFTF-B which will allow for experimental startup in the third quarter FY 1987. Funding is also requested to provide the necessary development support on the Fusion Materials Irradiation Test Facility (FMIT).

The FY 1985 level for Magnetic Fusion Energy Program Direction is $4.1 million. These funds are required to provide for the salaries, benefits, travel, and other expenses associated with 66 full-time equivalents.

Capital Equipment

The request for capital equipment is $33.4 million. This represents a decrease of $4.4 million in funding support and provides a level commensurate with the operating program described above. These funds are used to support the execution of the research program. Most of these funds will be used to procure hardware to enhance the effective and efficient utilization of the fusion experimental devices. The remainder of the funds are used to maintain existing equipment or upgrade and replace obsolete equipment.

Construction

The Construction budget provides $3.0 million for FMIT, $20.1 million for the MFTF-B, and $9.4 million for General Plant Projects (Table 3). FMIT will provide the capability to determine the effects of the fusion reactor neutron environment on candidate materials for future fusion reactors. The FMIT funding provides for the restart of construction activities and is predicated on international funding being available. MFTF-B is a thermal barrier tandem mirror device which will be the largest experimental facility in the world capable of investigating the mirror fusion concept. The requested funding for MFTF-B is based upon completion of facility construction in the fourth quarter of FY 1986. The increase in GPP funding of $2.9 million reflects the institutional requirement to reduce the overall backlog of general plant projects at Oak Ridge National Laboratory.

Table 3
Magnetic Fusion Energy
Budget Authority
($ in millions)

Construction	Prior Years	Request	TEC	Completion Date
Mirror Fusion Test Facility-B	217.5	20.1	246.6	FY 1986 (4th qtr.)
Fusion Materials Irradiation Test Facility	44.7	3.0	77.5 1/	FY 1988 (4th qtr.) 1/
General Plant Projects	N/A	9.4		
Total Construction		32.5		

1/ U.S. funds only. $67.0 million of anticipated foreign contributions, based on an assumption that an agreement is reached with foreign participants for them to fund 2/3 of the remaining construction costs, results in a total cost of $144.5 million.

Mr. **Lundine**. Thank you very much.

First of all, I would ask permission that we be allowed to ask additional questions in writing to those that will have the time to explore in person. Thank you.

Do you think that the TFCX will take the U.S. program beyond the JET and JT 60 in the sense that we—we will regain worldwide preeminence?

Dr. **Trivelpiece**. I don't know that we've lost our worldwide preeminence. There's certainly an approximate parity in several of the programs around the world at the moment. I don't think the United States has lost it.

TFCX could have parameters that would put it beyond what the present JET parameters are, or it could not. I think to some extent that would depend on precisely what technical goals and objectives are needed. And as is usually the case, there is some compromise between goals, objectives, price, circumstance, and so on.

Mr. **Lundine**. Well, let's stay with your discussion about the international situation. The European Community seems to be doing quite well in fusion R&D. Do you think that the United States has the resources to carry on with an aggressive fusion program, or do we need a joint venture?

Dr. **Trivelpiece**. No, in the sense in which you have posed the question, we don't. The United States clearly has the financial resources, the talent resources, to proceed with fusion at whatever pace might be appropriate. On the other hand, I think it has to be looked at in the context in which I was saying; namely, there may be other considerations that the heads of state that are involved in this may bring to the table and wish to have involved; that it isn't just simply the direct technical question that would be addressed at that level.

Mr. **Lundine**. I—your analysis internationally was—is appealing, to be sure. But do you have any idea how we would structure a joint venture?

Quite frankly, I think there's going to be some concern here on Capitol Hill because of some of the competitive factors, far remote in—in high technology, far remote from fusion. And I wonder if you've thought about how we might structure a joint venture so that, in effect, other friendly nations, but nevertheless competitive, may not take the benefit of our technology and sort of use it to leapfrog.

Dr. **Trivelpiece**. Well, there's a saying from the business that, "A deal, to be a good deal, has to be a good deal for everybody." So I think whatever it is that might be structured would have to be of benefit to the United States and whoever it might end up trying to enter into some partnership with.

So if we used the kind of example I was talking about—namely, suppose two of us agree that two objects costing some level of dollars are needed in some sequence in order to make progress, and that one party decides to build one and the other party decides to build the other and exchange information and agree to be bound by it and agree to complete it and make the facilities open and available and exchange all the information—this would be a cooperative phase. I would have no doubt but what, if they were both successful, there would occur a competitive phase, in which case then we would probably both shut down the information flow in order to try to pursue the market both domestically and foreign that we might be competing for.

But during this cooperative phase there does not seem to be a near-term market issue associated with fusion that should cause us to be unwilling to explore this as a possibility. And I think it has some potential benefit to the United States to look into this and at least there ought to be a sincere effort devoted to exploring it before we simply dismiss it out of hand.

Mr. **Lundine**. Well, I didn't mean by my question to suggest that I thought anyone would dismiss it out of hand.

Mr. Lowery?

Dr. **Trivelpiece**. Well, my—my boss wants me not to dismiss it out of hand, which is what I am getting at.

Mr. **Lundine**. I understand. [Laughter.]

Mr. **Lowery**. Thank you, Mr. Chairman.

Let me ask a very, very broad question. You said the program has been quite successful since, what, 1952. Were we to commit more resources, could we bring about a production reactor sooner? And how much more money?

Dr. **Trivelpiece**. The obvious answer is, "yes." And of course, infinite money won't make it happen tomorrow. And if you spend at less than a certain rate, well, you probably will never achieve success. And these programs have——

Mr. **Lowery**. Have you looked at a timetable for an appropriate level of funding and when we could realistically expect some results?

Dr. **Trivelpiece**. Increased funding could clearly accelerate the rate at which results could be obtained. However, if we could go out and order a fusion reactor tomorrow, place an order for one and have it available, there's still an insertion time into the economy that would be required, that any new technology takes some period of time to be accepted and to be made useful and—and gain general utilization levels.

Mr. **Lowery**. What are you looking at as our current target date at the current level of funding? 2020?

Dr. **Trivelpiece**. Well, it varies, and you have to make sure you know which question it is that you're asking and which question it is that I am trying to answer. I think that a demonstration of scientific breakeven is going to take place in the next few years. I think that it is entirely possible, should everything work, that in the mid-nineties, it would be possible to have an ignition experiment up and operating.

At that point, it would have to be looked at as to whether or not it would be in our interest to accelerate the rate of technology and engineering development and what was the economic climate that existed at that time. But I would believe that a 10-year period beyond the time ignition occurs should be sufficient to at least have identified the kind of technology problems and have some reasonable approaches to solving them. At that stage, it would clearly be a question of does it make commercial sense, will anyone buy them, is there anyone out there willing to build them? That's something over which we really have no control and requires a certain——

Mr. **Lowery**. You're looking at the mid-nineties for ignition?

Dr. **Trivelpiece**. I would think that would certainly be a possibility.

Mr. **Lowery**. Could that be brought about sooner? Would it be——

Dr. **Trivelpiece**. Yes, with some increased cost, and then you have to ask what do you distort in order to do that. And it requires some——

Mr. **Lowery**. OK.

Dr. **Trivelpiece** [continuing]. Husbanding of technical resources in a different direction.

Mr. **Lowery**. Dr. Trivelpiece, in the 1984 budget request, DOD stated that in late 1988 there should be sufficient data available to make a comparative evaluation between the Lawrence Livermore Tandem Mirror and Princeton's Tokamak, the confinement concepts.

In your statement you did not—at least what we received prior to testimony—there was no comment on that. Could you update, is that still the thinking of DOD?

Dr. **Trivelpiece**. I have tried personally to minimize the degree to which there is intense focusing on what might be referred to as the technical shootout at the earliest possible time. I think it's important in this program, and past experience I think supports this kind of a view, to maintain things and allow them to proceed at their own pace and their own scientific arena. I think that's true with the Mirror program and the Tokamak program and several other elements of the fusion program.

It's also consistent with the kind of advice we have been receiving recently from ERAB and the Magnetic Fusion Advisory Committee.

I'd go back to point out as an example—in this case, if we went back to 1946—and asked what kind of nuclear reactors would we have in the United States, we'd either have fast metal breeders or we would have HTGR's, that the light-water reactors at that stage hadn't been made successful, because of certain materials problems. So in a short span of one year later, an option which was at that stage only being considered experimentally has turned out to be the commercial option.

Perhaps some of your constituents at GA Technologies would—might have hoped that hadn't have happened. That's another problem. [Laughter.]

Mr. **Lowery**. Well, we're looking to the future, and we're looking at little things like HTGR's as well as fusion. So we're eternally optimistic in San Diego.

How about—I mean you mentioned the international cooperation. Is there sufficient funding being provided by other countries to their fusion programs that make the international cooperation make more sense——

Dr. **Trivelpiece**. Well, certainly in the——

Mr. **Lowery** [continuing]. In the honest tradeoff of information?

Dr. **Trivelpiece**. I believe that the exchange of information under the present set of circumstances is quite outstanding, that we have benefited a great deal from activities that take place in other countries, and including the Soviet Union. The U.S. fusion program has benefitted.

Sometimes people ask what we get out of the Russians in these kinds of things. And I would like to point out that we study Landau damping using the Vlasov equation to explore plasmas that are contained in Tokamaks heated by gyrotons; the electrons are explored to some extent by studying the Cherenkov emission that they have. Those are all terms that we have acquired from the Russian language. And other than that, we really haven't gotten too much from the Soviet Union in the fusion program. [General laughter.]

Mr. LOWERY. How about other countries?

Dr. TRIVELPIECE. Other countries have contributed quite well. I think the European Communities, as a collection, have done an outstanding job in developing fusion, fusion experimental devices. And they have shared the information with us quite openly.

I am not aware of anyone being particularly closed with respect to providing information.

Mr. LOWERY. Is the level of funding similar to ours, or is it more or less?

Dr. TRIVELPIECE. I think if you take the European Coummunities collectively, it's about the same as the United States. Japan is rapidly approaching the United States, and the Soviet Union is about like the United States.

But I think Dr. Clarke maintains a closer perspective on that than I do, if you'd like to refine that.

Mr. LOWERY. Are you optimistic about a breakthrough of cooperative agreements and sharing of information?

Dr. CLARKE. Well, as Dr. Trivelpiece said, we're being very successful in exchanging information now. As far as any breakthroughs, I'm not——

Mr. LOWERY. Well, divvying up the pie, if you will, so there's not continued duplication.

Dr. CLARKE. That's—that's very difficult. It's going to take a long, hard negotiating process in order to achieve that. And I think Dr. Trivelpiece indicated that if the heads of state give an instruction that it shall be done, it'd be a lot easier.

Mr. LOWERY. Mr. Chairman, one final question.

The data processing network for the magnetic fusion program, I know you've been expanding it for others. What's in the fiscal year 1985 budget to expand it? And how is the expansion going?

Dr. TRIVELPIECE. The expansion that we had planned required a—submitting a reprograming request to the committees here in the Congress. The appropriations committee in the House is still holding our reprograming letter and have not approved it.

That would entail adding an additional class 6 system to the magnetic fusion energy computer center at Livermore and making it available, in part, to the fusion program and, in part, to the energy, other energy research programs, such as high-energy physics, nuclear physics, and the like.

If we would plan to continue that into the 1985 time frame, there the funding has been requested in the basic energy sciences program. It's not in the fusion program.

We're managing these things kind of as a collective activity for the whole supercomputer access and development effort, and the fusion computer center is being utilized for the time being to assist in that activity.

Mr. LOWERY. Thank you, Al.

Thank you, Mr. Chairman. I know we're running short on time.

Mr. LUNDINE. We have a vote on the House floor. I think it's a final passage vote. Therefore, it'll be our last.

I think what we'll do, if you're able to stay a slight bit longer, is take about a 10-, at the very most, 15-minute recess, and return and have a few more questions. Is that satisfactory?

Dr. TRIVELPIECE. Fine.

Mr. LUNDINE. The committee will be in recess for 10 minutes.

[Short recess taken.]

Mr. LUNDINE. Thank you very much for your patience with our up-again, down-again sort of existence.

The Energy Research Advisory Board report essentially recommends, as we see it, deemphasizing the engineering thrust while continuing the scientific emphasis during this period of restricted budgets. Do you agree with that position?

Dr. TRIVELPIECE. You have to look at the context in which the ERAB statement was made. There was a period a few years ago in which the Magnetic Fusion Energy Act came out—and the gentleman you were just talking to, Mr. McCormick, was, of course, one of the authors of that—and what that did was to envisage that the TFTR, or the break-even experiment, would be successful and postulate that if that is the case, then we should begin an accelerated program of engineering development such that when that event takes place, we're prepared to take maximum advantage of it and go immediately to a reactor level activity.

The combination of activities that involved the department's budget leveling out and and other things had said that we shouldn't do engineering instead of physics. That's quite—quite an oversimplification. But basically, that to not start those engineering activities that anticipate a success in the scientific program. And I think that's the context in which ERAB is making the statement.

They're saying:

> "Don't do the engineering work, don't do the accelerated engineering work that anticipates any of these results, but do continue the kind of engineering and technology development which has characterized the fusion program for many years.

But most of that work now is in response to the direct scientific needs of the program. It is related to the technology developments that are needed for the next experimental programs, but not the general engineering kind of thing. It's not that we should stop doing engineering and stop doing technology development.

I take it in that spirit, and in that spirit I agree with their statement in the present budget climate.

Mr. LUNDINE. In a related matter—but I'm not sure that I've—if you have answered it, I'm not sure I understood—the TFCX represents a scientific and engineering challenge of major proportion. Do you have, does DOE have, a position on a configuration for TFCX? And in particular, would that configuration facilitate future upgrade in an effort to minimize the total capital cost of the next two devices?

Dr. TRIVELPIECE. To some extent, the question contains one element of the answer; and that is, by configuration, do you mean Tokamak versus Mirror? Since it's called the Tokamak fusion core experiment, we would presume it to be a Tokamak-like device.

On the other hand, there are other considerations with respect to magnets and methods of heating and so on. There have been different designs considered. And I think perhaps Dr. Clarke would like to be asked to address part of that question.

Mr. LUNDINE. Thank you.

Dr. Clarke.

Dr. CLARKE. At the present time, we have about eight variations on the basic—eight variations of the Tokamak that could achieve the basic goals of the TFCX. And each of these approaches to achieving those goals has varying capabilities for upgrade. And I might add, they also have varying costs.

So the effort over the next year and a half is aimed at sorting out these—these configurations to try to find one that has the appropriate compromise between upgrade capability, which, of course, is desirable, and also minimizing the cost, which is desirable. And, unfortunately, these are values that go in different directions.

Mr. LUNDINE. So my preconceived notion that it's an either-or: Either you're going to have the upgrade capability or not is limited, at best.

Dr. CLARKE. Well, it's a little bit more of a continuum than you——

Mr. LUNDINE. And you have various options within that.

Dr. CLARKE. That's right. There are tradeoffs between costs and degree of technology.

Dr. TRIVELPIECE. I would certainly hope that whatever is built would have the capability of being upgraded to take advantage of discoveries and developments. It would seem to me to be a mistake to put together something that didn't have that feature.

Mr. LUNDINE. Well, then the only other question that I have that I want to ask you other than for the record—but the Chair has a number of questions which she particularly will hope you'll answer in writing—is one of interest to me. Can you tell me whether there's any funding in basic research directed toward inertial confinement fusion for civilian applications?

Dr. TRIVELPIECE. As part of the magnetic fusion program here today or part of the——

Mr. LUNDINE. Any at all?

Dr. TRIVELPIECE [continuing]. Basic sciences——

Mr. LUNDINE. Any at all?

Dr. TRIVELPIECE. I'm not sure I understand the context in which the question is being asked. The inertial confinement fusion program is supported through the defense program's activity in the Department. I am moderately familiar with the collection of activities that are going on. I think there are among the activities being supported, certainly things that could be properly characterized as basic research into the interaction of lasers and electron and ion beams with matter. The result——

Mr. **Lundine**. But as you see it, it doesn't have civilian application, because it's being done on the defense side of DOE?

Dr. **Trivelpiece**. No, that doesn't necessarily follow. There have been a collection of facilities that were built through the auspices of the defense program. It has—as a matter of policy, Herm Roser, who was the Assistant Secretary for defense programs in the last couple of years, has said that those facilities should be completed and operated and utilized to explore the kinds of physics and interactions of laser-light with matter that can be done with them, and that the focus of that activity on the production of civilian power should at least be held in abatement for the time being while the military application aspects of it are being explored.

It does not make a statement that it can't happen, won't happen, or shouldn't happen, but just that that is the defense program's attitude with respect to how they wish to see it managed for the time being.

Mr. **Lundine**. So I can conclude, therefore, that you're not funding any research in that area?

Dr. **Trivelpiece**. There is a program which is managed through the basic energy sciences program called heavy ion fusion, in which—to use conventional accelerator technology to understand how conventional particle accelerators can be used to create a beam of particles that has the right energy and the right density and the right current to cause pellet implosion to take place, that is being looked upon to some extent as being more of a civilian energy potential activity.

It's currently supported at $5 million in the basic energy sciences program. And we're reviewing it. It was transferred over to us from the defense programs activities last year. We've agreed to manage it and review it and take a look at it and assess its potential.

Mr. **Lundine**. And you haven't made any commitment as far as funding it for next year?

Dr. **Trivelpiece**. $9 million.

Mr. **Lundine**. $9 million is——

Dr. **Trivelpiece**. It's being requested in 1985 as part of the basic energy sciences program.

Mr. **Lundine**. If there are no other questions, I want to thank both of you very much for this testimony and for your willingness to answer additional questions in writing.

It may be a subject that takes a little bit of vision, but the work you're doing in fusion could be of enormous importance to our children and our children's children, and certainly deserves our attention and a commitment.

Thank you very much.

Dr. **Trivelpiece**. I couldn't agree more.

Thank you.

Mr. **Lundine**. The committee is adjourned.

[Whereupon, at 3:03 p.m., the subcommittee was adjourned.]

1985 DEPARTMENT OF ENERGY AUTHORIZATION

WEDNESDAY, FEBRUARY 29, 1984

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Research and Production,
Washington, D.C.

The subcommittee met, pursuant to notice, at 1:05 p.m., in room 2318, Rayburn House Office Building, Hon. Marilyn Lloyd (chairwoman of the subcommittee) presiding.

Present: Representatives Lloyd, Walker, and Lujan.

Staff present: Dr. John V. Dugan, staff director; Ron Williams, technical consultant; Dave Clement, minority counsel; and Bill Harvey, minority energy coordinator.

Mrs. Lloyd. The subcommittee will come to order.

Good afternoon.

As I told the AIF conference attendees the other day, the single major issue in this budget is the question of the initiation of a focused effort on the TFCX. We are going to hear a lot about this device this afternoon, and I don't think it is any surprise that the message that we have from all quarters is go. This long pulse, ignition device will take the U.S. program beyond the late 1980's capability of both the OECD JET and the Japanese JT-60.

I should also repeat my position on the need for additional funding for TFCX in this difficult budget climate for fiscal year 1985. I am mindful of deficit concerns, but I will not allow this subcommittee to be such a victim of the budget climate that we make precipitous, short-sighted decisions.

There have been some very positive developments in the magnetic fusion program over the past several years which have led to an enhanced climate of increasing confidence. The Tokamak and Mirror programs have shown significant progress including the recent very fine performance of TFTR and this contributes greatly to program competence. Industrial contributions to the program have been significant. The national laboratory/industry and laboratory/university partnerships have been greatly strengthened. The university role has been enhanced and MIT's TARA is now the flagship for this university support role.

The program continues to attract the very best people from the national labs, industry and the universities. The Fusion Engineering Design Center has obtained significant credibility and should have a stronger role in toroidal planning activities over the next several years. Privately funded, high risk, potentially high payoff work is going ahead on compact high density devices.

I should think it would amount to a gesture of bad faith for the United States to stretch out and delay its commitment to TFCX simply because of tight budgets. I also do not feel that we can accept flat budgets for this program for the outyears because just modest additions would allow the United States to retain international preeminence.

I am looking forward to hearing from an excellent group of witnesses on all these matters. Our first panel of witnesses includes Dr. Harold Furth, Dr. Tihiro Ohkawa, Dr. Charles Baker, and Dr. Carl Henning. Gentlemen, we welcome you to our subcommittee hearing this afternoon. Please proceed.

I would like to say at this point that we will have the complete statements of all of our witnesses this afternoon will appear in full in the record. Any of you may proceed to summarize as you wish.

I would also like to state that the opening remarks of the minority members will also be included in the record.

[The prepared opening remarks of Mr. Winn and Mr. Walker follow:]

OPENING STATEMENT BY THE
HONORABLE LARRY WINN, JR.

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
FY 1985 DOE AUTHORIZATION FOR MAGNETIC FUSION
FEBRUARY 29, 1984

THANK YOU MADAM CHAIRMAN, FOR THIS OPPORTUNITY TO WELCOME OUR WITNESSES TODAY ON MAGNETIC FUSION. WE HAVE ALREADY DISCUSSED THE PROGRAM WITH THE DEPARTMENT OF ENERGY AND TODAY WE LOOK FORWARD TO HEARING FROM A NUMBER OF PEOPLE INVOLVED AS CONTRACTORS IN THE PROGRAM.

THE MAGNETIC FUSION PROGRAM REPRESENTS ONE OF THOSE ADVANCES IN TECHNOLOGY FOR WHICH FEDERAL FUNDING IS VERY APPROPRIATE. THE HIGH-RISK, LONG-TERM NATURE OF THIS PROGRAM, WHICH PROMISES SUCH SUBSTANTIAL PAYOUT, CERTAINLY DESERVES FEDERAL SPONSORSHIP. THE AMOUNT OF FUNDING REQUESTED BY THE DEPARTMENT OF ENERGY FOR FY 1985 APPEARS TO BE QUITE ADEQUATE TO PROVIDE HEALTHY SUPPORT.

THE UNITY OF SUPPORT FOR THE TOKAMAC FUSION CORE EXPERIMENT IS VERY IMPRESSIVE. NEARLY ALL THE TESTIMONY RECEIVED FOR THIS HEARING HAS ADVOCATED SUPPORT FOR THIS FACILITY TO FOLLOW THE TOKAMAC FUSION TEST REACTOR. OTHER TESTIMONY WHICH STRUCK A

RESPONSIVE CORD IS THE STATEMENT BY SEVERAL WITNESSES MAKING A CASE FOR INDUSTRIAL PARTICIPATION. AS YOU KNOW, MADAM CHAIRMAN, CLAIMS IN PRIOR YEARS THAT INDUSTRY DID NOT HAVE ADEQUATE OPPORTUNITY FOR PARTICIPATION HAD CAUGHT MY ATTENTION. I ALSO WELCOME THE SUGGESTIONS BY OUR WITNESSES THAT UNIVERSITIES TAKE A CONTINUING AND INCREASING ROLE IN THE PROGRAM.

THE COMMITTEE CERTAINLY WELCOMES INNOVATION AND CREATIVITY. IF, AS MAY BE THE CASE WITH THE TOKAMAC FUSION CORE EXPERIMENT, DOE HAS FOUND REASON TO CHANGE PRIORITIES, WE WILL WORK CLOSELY WITH THEM TO PROVIDE THE FLEXIBILITY THEY NEED, WORKING WITHIN THE REQUESTED FUNDING LEVEL. I MAKE A VERY STRONG REQUEST TO PARTICIPANTS IN THE MAGNETIC FUSION PROGRAM THAT, IF NEEDS ARE SEEN TO INCREASE SUPPORT OF ONE PORTION OF THE PROGRAM, THEN WE SHOULD BE ADVISED OF THE SOURCE OF THOSE FUNDS ELSEWHERE IN THE PROGRAM. A DEEP CONCERN FOR THE BUDGET DOES NOT ALLOW US TO CONSIDER ADDITIONAL FUNDING.

THANK YOU, MADAM CHAIRMAN. WE LOOK FORWARD TO HEARING FROM OUR WITNESSES.

OPENING REMARKS OF
U. S. REPRESENTATIVE
ROBERT S. WALKER
FEBRUARY 29, 1984

GOOD AFTERNOON, MADAME CHAIRMAN, AND I WOULD LIKE TO JOIN YOU IN WELCOMING OUR WITNESSES BEFORE THE COMMITTEE THIS AFTERNOON.

OUR TOPIC THIS AFTERNOON IS MAGNETIC FUSION AND IT IS AN AREA IN WHICH THERE IS CONSIDERABLE PROMISE FOR THE FUTURE. IN AN AGE WHEN WE IN THE CONGRESS MUST BE CONCERNED ABOUT THE ENERGY NEEDS, NOT ONLY OF THE UNITED STATES, BUT ALSO OF THE NEEDS OF THE REMAINDER OF THE FREE WORLD, IT IS ENTIRELY PROPER THAT THE DEPARTMENT OF ENERGY SHOULD BE DOING RESEARCH ON THE VERY FRONTIERS OF THESE TECHNOLOGIES.

IN THE VERY NEAR TERM WE MUST BE CONCERNED ABOUT THE CONSEQUENCES OF POSSIBLE DISRUPTION OF ENERGY SUPPLIES FROM THE MIDDLE EAST BASED ON THE CONTINUING INSTABILITY IN THAT AREA. WITHIN THE PAST TWENTY-FOUR HOURS THE IRANIAN GOVERNMENT HAS THREATENED TO CLOSE THE VITAL STRAIGHTS OF HORMUZ. WHILE AMERICAN DEPENDENCE ON OIL FROM THAT AREA HAS BEEN GREATLY DECREASED SINCE THE ARAB OIL BOYCOTT, WE STILL HAVE MAJOR RESPONSIBILITIES TO OUR EUROPEAN ALLIES AND OUR JAPANESE ALLIES WHO ARE DEEPLY DEPENDENT ON THAT OIL.

IN THE LONGER TERM, WE ALL REALIZE THAT OIL IS A LIMITED RESOURCE, AND THAT WE MUST DEVELOP NEW AND INEXHAUSTABLE SOURCES OF ENERGY. MAGNETIC FUSION SHOWS GREAT PROMISE FOR COMMERCIAL POSSIBILITIES IN THE NEXT CENTURY. BUT WE MUST ALSO REALIZE THAT WITH THESE LONGER TERM PROJECTS DEALING WITH THE LEADING EDGE OF HIGHLY ADVANCED TECHNOLOGIES THERE ARE BOTH SCIENTIFIC AND POLITICAL PROBLEMS. THESE PROGRAMS HAVE TREMENDOUS COSTS AND, AT LEAST IN THE SHORT RUN, THERE IS LITTLE POSSIBILITY OF COMMERCIAL COMMITMENT TO THE RESEARCH.

I ALSO HAVE A DEEP AND ABIDING CONCERN THAT IN THE AREA OF MAGNETIC FUSION WE MAY REACH THE POINT WHERE WE MAKE MAJOR COMMITMENTS TO A SINGLE VERY LARGE DEMONSTRATION PROJECT WHICH WILL THEN BECOME THE FOCUS FOR THE ANTI-NUCLEAR ACTIVISTS. WE ALL SAW THAT THE CLINCH RIVER PROJECT BECAME VULNERABLE WHEN THE OPPONENTS WERE ABLE TO FIND WAYS TO STRING THE PROJECT OUT, YEAR AFTER YEAR, UNTIL ESCALATING COSTS EVENTUALLY MADE THE PROJECT ECONOMICALLY UNFEASIBLE.

IT APPEARS TO ME THAT WE HAVE A NEED FOR SOME SHORTER-TERM GOALS WHICH WILL BE READILY ACHIEVABLE, AND WHICH WILL HELP TO MAKE THE PUBLIC AWARE THAT THIS TECHNOLOGY WILL WORK.

YESTERDAY WE EXAMINED THE URANIUM ENRICHMENT ACTIVITIES OF THE DEPARTMENT OF ENERGY AND THOSE WHO ATTENDED THOSE HEARINGS PROBABLY PERCEIVED THAT THERE IS A GROWING SENSE, BOTH WITHIN THIS COMMITTEE, AND IN THE CONGRESS AT LARGE, THAT OUR ENERGY RESEARCH PROGRAMS ARE IN DIRE NEED OF RESTRUCTURING. MY OWN VIEW, AND THAT OF OTHER MEMBERS OF THE COMMITTEE, IS THAT:

* RESEARCH AND DEVELOPMENT ACTIVITIES MUST BE BETTER TARGETED TO MEET AMERICA'S ENERGY NEEDS INTO THE FORSEEABLE FUTURE.

* WE CANNOT CONTINUE TO ENDLESSLY FUND PROGRAMS THAT RESULT ONLY IN FINDINGS THAT MORE RESEARCH IS REQUIRED. THERE MUST BE A POINT AT WHICH RESEARCH IS EITHER SUCCESSFUL, OR IT IS ABANDONED IF IT IS OBVIOUS THAT IT WILL NEVER SUCCEED.

* THERE SHOULD BE A MAJOR RE-EXAMINATION OF PROGRAMS AND PRIORITIES. WE CANNOT FUND PARRALLEL PROGRAMS IN PERPETUITY. IN MANY OF THESE PROGRAMS THERE MUST BE A POINT WHERE TECHNOLOGIES COMPETE AND THE WINNER GETS CONTINUED FUNDING, AND THE LOSER IS DROPPED.

- THERE ARE AREAS SUCH AS URANIUM ENRICHMENT WHERE THERE ARE OVER-RIDING CONSIDERATIONS SUCH AS NUCLEAR SAFEGUARDS WHICH PLACE THE ACTIVITY INTO THE LEGITIMATE SPHERE OF GOVERNMENTAL ACTIVITIES. HOWEVER, THERE IS ABSOLUTELY NO REASON WHY THESE PROGRAMS SHOULD NOT BE RUN IN A BUSINESSLIKE MANNER. THIS INCLUDES MAKING DEVELOPMENTAL DECISIONS BASED ON ADEQUATE MARKETING DATA, AND COMPETITIVE PROCUREMENTS.

THIS YEAR WE EXPECT TO BE CALLING ON THE DEPARTMENT TO UNDERTAKE A MAJOR NEW EFFORT TO DEVELOP INHERENTLY SAFE REACTORS. IT APPEARS TO ME THAT THERE IS MORE THAN SUFFICIENT MONEY IN THE PRESIDENT'S BUDGET REQUEST TO CONTINUE ON-GOING PROGRAMS WHICH ARE MERITORIOUS, AND TO UNDERTAKE THIS NEW INITIATIVE. I, AND I BELIEVE OTHER MEMBERS OF THIS COMMITTEE, WILL BE WILLING TO ASSIST THE SECRETARY OF ENERGY IN FINDING THE FLEXIBILITY TO REALLOCATE HIS RESOURCES TO MEET WHAT WE SEE AS CHANGES IN OUR ENERGY R&D REQUIREMENTS.

I MUST CAUTION THE ENERGY R&D COMMUNITY, HOWEVER, THAT THERE IS A NEW SENSE AROUND HERE THAT WE MUST CONTROL DEFICITS. THIS WILL NOT BE A YEAR IN WHICH THE CLIMATE WILL BE FAVORABLE TO BUDGET ADD-ONS. WHAT WE WILL BE LOOKING FOR WILL BE TECHNOLOGIES WHICH HAVE A PROMISE OF REASONABLE PAYBACK, TECHNOLOGIES WHICH WILL ATTRACT FUNDING FROM OTHER NATIONS OR FROM THE PRIVATE SECTOR, AND FOR TECHNOLOGIES IN WHICH THERE IS A SUBSTANTIAL POSSIBILITY OF MAKING MAJOR ADVANCEMENTS.

THIS IS NOT TO SAY THAT WE CANNOT SUPPORT THE FUNDING LEVELS CONTAINED IN THE PRESIDENT'S BUDGET, IN GENERAL I BELIEVE THAT THOSE LEVELS ARE REASONABLE. BUT WE WILL HAVE TO FIND WAYS WITHIN THAT REQUEST TO GET THE MOST RESEARCH ACCOMPLISHED.

Mrs. LLOYD. Please proceed, Dr. Furth.

STATEMEMT OF DR. HAROLD P. FURTH, PLASMA PHYSICS LABORATORY, PRINCETON UNIVERSITY

Dr. FURTH. Mrs. Lloyd, members of the committee, ladies and gentlemen, I am extremely pleased to be here. It has been a little over 12 years since I was last asked to speak on fusion research to Congress, and I hope you will excuse me if I indulge in a bit of nostalgia and recall what it was that we were talking about the last time.

In November 1971, when I last had the privilege to speak on this topic, what we showed the committee was this viewgraph which I have cleaned up a little bit and decorated with new things. What you see here is the Lawson diagram, the ion temperature on the vertical axis. We are trying to get to 100 million degrees here. On the horizontal axis is the energy confinement parameter.

Down in the left-hand corner, there is a little brown cloud marked "small toruses." I remember very well in the early days of the fusion program, we had an exciting time trying to explain to the Congress how it was that we were going to get—from this little cloud—up three orders of magnitude in temperature, four orders of magnitude in confinement, to the goal marked reactor. I would like to compliment the Congress for the extraordinary patience, perception, and understanding that it showed in thinking that this was remotely conceivable—which, in fact, they did.

By 1971, the picture had changed. We had just received the news and digested the news of the Soviet work on Tokamaks, on the T3, and had repeated it on the ST Tokamak in the United States. The subject of those hearings was: Well, that looks very encouraging and what do we do now? These were very lively hearings. We had heavy attendance, including Senator Baker who was after me every moment—I just reviewed the transcript—ferociously pursuing me and at the same time making highly intelligent and supportive remarks, and Mr. Hosmer and Senator Symington, and other good people in the same style.

What we sought to persuade them of and, I think, did persuade them of was that something could be done beyond the amazing accomplishment of the Soviet Union, that we should undertake a forward looking program and we would be able to push on. During the 1970's, we promised something very like the red line that you see there which was actually accomplished with the machines of the 1970's. Further, we had the temerity to say we thought that the new Tokamak would carry us all the way to the final goal, ignited burning plasma.

To discuss this in a little more detail, could you give me the next viewgraph which is an enlargement of the upper right-hand corner. Here again you see the Lawson diagram. Here is the amazing T-3 point of 1971, which has now sunk to the bottom. Here are the experiments which were built as a result of the hearings in 1971. Here is the new TFTR, which went into operation last Christmas and which already is producing very satisfactory experimental results, as you will see.

Beyond that is the real reactor core. At the moment, we call this the Tokamak Fusion Core Experiment. There won't be a great succession of ever new targets beyond that: This is it. This is where you want to sit for a real reactor. When you progress from here to make an engineering test reactor or a fusion power demonstration, you do not change the core. What you do is you wrap the nuclear technology around it, so to speak. It is not quite as simple as that, but basically that is it. I will say a little bit more about the TFCX as I go along.

In the next slide, one more bit of nostalgia. At the top you see the testimony of 1971, the experimental status at the time, what we thought we could accomplish in the enormous new PLT machine which we were all rather frightened of. It was one of the biggest things ever seen, and is now, of course, one of the smaller things that are being seen. Finally, here was our promise that perhaps this would lead to a real ignited reactor core and what its characteristics would be.

In the present testimony, let me draw your attention to the line, "experimental status", where I have listed not the results of a single experiment, but the best results obtained in various experiments in the U.S. program which, incidentally, also are the best results in the world. On PLT, the temperature is now up to 7 keV, not 2.5. The initial results of TFTR are up to 300 milliseconds. Alcator C, as you heard last fall, achieved the amazing result of an NT value equal to eight times ten to the 13th power almost there in the reactor regime, and D–III achieved almost the desired 5 percent of beta value, which is the entry point to a reactor.

So, to make a long story short, we are now at a point where in individual experiments, we have come not within a factor of 1,000 or 10,000, but within a factor of 3 of each of the key parameters that you need to make a real reactor core.

The next step in TFTR will be to put that together and start doing some of these things simultaneously. It is our expectation that TFTR is going to be good enough to get to the breakeven point. Beyond that is the TFCX where we hope to put it all together and have the real thing.

In the next slide, I can't resist showing you some of the results of initial experimentation in TFTR. This was done at half magnetic field and at half current, but, as you see, it is not too bad. What is plotted is the energy confinement time, which, in a real reactor, will have to be about a second, depending on exactly how you do it, maybe a little bit more or a little bit less. That used to seem a great deal when we had 5 microseconds or 10 microseconds of energy confinement time, but now you see the top points are up to almost three-tenths of a second.

What is perhaps even more interesting is how this compares with previous thinking of how these machines would scale the size. In the 1971 testimony, we said with great trepidation that from first principles it seemed likely that the confinement time should improve something like the square of the size, because that is what heat diffusion processes usually do. However, we said it rather faintly because there were some prepared to argue that it might only go up linearly, and it was only a hope. Yet, that was the hope for getting to a Tokamak ignition regime.

The remarkable thing is that the previously held scaling law, the so-called INTOR scaling which was used in the international reactor study in Vienna, indeed only went like the square of the size. However, what is happening is that the confinement is improving like the cube of the size. It is not entirely unexpected. Experiments at MIT already 2 years ago saw inklings of it, but it is nice to see it so dramatically.

That in itself is a most encouraging start. I should add that our cousins in England on the JET machine have obtained very similar results. That is a machine which is somewhat larger. They will be able to run even higher currents. I believe we will be able to get over half a second in this regime, and maybe they will get 0.6 seconds or even more before too long.

I have struck a very encouraging note here, but let me temper it a little bit. This is in the ohmic heating regime. The real proof of the pudding will come starting this summer and getting increasingly more exciting in the course of the year, until finally we see the full truth dawning next year, and that is when we now intensively heat this plasma with neutral beams. At the moment, we are up to almost 20 million degrees, but we would like to go to 100 million and then, just for good luck, up to 200 million.

As we go up there, it is entirely possible and, indeed, consistent with present results, that the scaling laws may change. We cannot definitely predict how they will go. So, the real excitement, the really relevant thing is yet to come starting this summer, and then continuing for the next year or two.

In the next slide, you see a viewgraph of TFTR as it looks today. The big box on the right did not appear on previous viewgraphs, that is, the neutral beam injector. We now have got two of them installed. We are now getting ready for operation beginning in late April when we hope to start running the machine again. As I say, perhaps as early as this summer, we will have the first inklings of results.

Now, why is it so particularly important to have those results and, particularly, to have them soon? Because as soon as one sees how the scaling law really goes in detail in this higher temperature regime, one will for the first time be absolutely certain exactly what it takes to do the ignition experiment, to build the real reactor core. Therefore, it is an enormous advantage to have that come early, in this case, about 3 years before anybody else in the world can do it, and, at the same time, to have the wheels spinning, to be reviving up a team that is doing a conceptual design, that is folding in the data as it arrives, readjusting its course. That can be ready in 1986, to start doing detailed design, and in 1987, to profit from this and go ahead and build it. When you know what it is and how it is best done, to go for that great prize, the first ignition experiment.

On the next slide, a few words about the ignition experiment, the TFCX. It is a national project which, at the moment, is led by Princeton. It has participation from the other major laboratories. Starting this fall or sooner if possible, it will have extensive participation from industry. It has a national advisory committee to help us decide which of the many variations that we could use will be the best.

Just so it isn't completely nebulous, let me indicate some particulars. An ignited Tokamak plasma will tend to produce 200 to 400 Mw of fusion power. In the interests of learning all the most essential things, we would like to have it burn a few minutes, or maybe even 5. If you multiply those you will see we are talking about an experiment that produces 100 gigajouels of energy per pulse, so this is not a trivial laboratory test. This is a real reactor.

In regard to what is needed to do that, as I said, we will not know absolutely until we see the results from TFTR and from JET and from Doublet and some of the other machines, but the present betting is it will need a current of about 8 to 12 MAmps, two or three times the maximum of the present generation of large experiments, and a magnetic field somewhere in the range of 4 to 6T, like TFTR.

Will this machine be enormously large? No, it will be about the same size as TFTR and JET, just shaped a little differently, a little stronger magnetic field and higher current.

Where do we go from there? In terms of making fusion power, we don't have to go anywhere, because this is it. In terms of making a useful reactor, there are various ways to go. One is to put a blanket around it, with shielding, and superconducting coils. That is sort of the conventional approach. That will lead to a machine of about twice that size. That is a possibility and it looks like a thoroughly viable thing. I think you will hear about it more from other speakers.

It is interesting that one can also go the other way. If we depart from things for which there is a very solid experimental base, and look, as we should, to things that are a little more speculative but may have a very large payoff, then that is symbolized by this little thing at the bottom which has the same power production as the big one. It is mechanically much simpler, because we have eliminated the coils that go around it. This is just a little thing like a smoke ring with the coils at some distance. This is what is known as a compact Taurus. Whether that will, indeed, thrive our experiments will show, but it is entirely possible. I would say that this is no crazier from the present perspective than that was from the perspective of 1960. The pace is picking up, so maybe to bring a compact Taurus within the realm of reality won't take 20 years.

That then is the broad perspective. On the one hand, there are things we feel confident we will know how to do. On the other hand, there are great and inspiring things to look forward to.

If I haven't outstayed my welcome, I would like to show you one last slide. This is sort of a philosophical slide which, however, I think might be helpful for subsequent discussion.

The point here is, as I have already suggested in regard to the previous slide, there is a kind of tradeoff between virtuosity in physics and the difficulty of doing the engineering. I have plotted the engineering difficulty on the vertical axis and the physics virtuosity on the horizontal.

For example, before 1970, the engineering difficulty of making a fusion reactor was infinite. The magnetic field strengths and sizes one would have had to make in order to make a working reactor, just extrapolating from what we were able to do, would have been out of this world, not even worthy of consideration. The exciting event around the time of the 1971 hearings was that all of a sudden the engineering difficulty had become finite. It was great; it was not something that you would want to rush right out and build, but you could describe it. In the universities and national laboratories, there began feverish reactor design activities, the sheer joy of being able to describe something that was real and conceivable.

Those objects were enormously large, but they were credible. What has happened during the past decade is that those sizes have come down. They have come down essentially because our ability in plasma physics has increased, so we were no longer up against it having to visualize very primitive objects. We were able to apply this increasing skill in order to make the engineering easier. That is a process which, extrapolating from the ideas we have today, I think is extremely likely to continue.

Now, the next question is, well when is it good enough? Here, I identify three main targets. In the first place, without having acquired any enormous skill, considerable but not enormous skill, on plasma confinement, one may be able to make a fission/fusion hybrid to breed nuclear materials. The reason that is easy is that it doesn't call for very large energy multiplication. For example, TFTR. Taken as it is, it is good enough.

Therefore, one strategy would have been to say, "it's commercial is good enough. Let's go directly for that and let's start selling it. We will get Q=1 in TFTR in 1986 and in 1987 we will put out a brochure and start selling it."

The program has decided not to do that on the basis of the argument that it is not very much more difficult to make a burning plasma with an infinite energy multiplication like TFCX. That is not much more difficult than to make it with Q=1 on TFTR, and the advantages of pushing on further on are very great because if you have a pure fusion reactor, you have great environmental benefits, you have great benefits on the proliferation question, and so it would be just foolish to stop short and not go that last piece of the road.

Therefore, the plan is to drive from the origin in 1970 towards the point which we conceive now as being a reasonable aiming point for a commercially attractive pure fusion reactor around the period early in the next century, maybe 2020 or something of that sort and assuming that the price of uranium is going to go up and other timely factors of that sort.

We could be mistaken and, for environmental reasons, that level may rise and make it easier for a fusion reactor to qualify. We could be mistaken, and for economic reasons, things may be tougher. That is why it is rather relevant that we are not aiming here as an end point. The object is first to go to a real reactor and then to progress along the line of tradeoff between engineering and physics, coming ever down towards more attractive regimes. Needless to say, if it is found that the economic objective gets tougher as we go along, there isn't going to be this corner in the curve. We are going to round it off.

Somewhere in what I consider to be the far future, is the far more difficult target of making a fusion reactor work, not with deuterium-tritium, but with advanced fuels. Pure deuterium is not such a difficult target although I think one could show that if you can make a pure deuterium reactor, then you are automatically skilled enough to make a simply superb, economically competitive DT reactor, and the same goes further on down the line.

I think we should all hope that sometime in generations to come people will be able to realize the goal of neutronless fusion. At the moment, we do not know how to do it, but I think that is a very good thing to aim towards. If we ever acquire that kind of virtuosity, it will follow, incidentally, that we will be able to make superb deuterium tritium reactors, highly economically competitive, and DD reactors.

Thank you very much, Madam Chairman.

[The prepared statement of Dr. Furth follows:]

PROGRESS IN TOROIDAL CONFINEMENT RESEARCH

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PRESENTED TO
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

FEBRUARY 29, 1984

Progress in Toroidal Confinement Research

The last time I testified on fusion to a Congressional Committee was on November 10, 1971. The hearings were attended by Senators Baker, Pastore and Symington, and by Representatives Hansen, Hosmer, Price and Young. As you might guess, they asked some penetrating questions. On examining the record, I am happy to report that the answers they received twelve years ago have held up fairly well.

The main topic at the 1971 hearing was the remarkable performance of the Soviet T-3 tokamak, which had recently been replicated in experiments on the ST device at Princeton. A broad new experimental program was proposed, to advance U.S. tokamak research towards a working fusion reactor.

The roadmap that measures progress towards the reactor regime is known at the Lawson Diagram. An edited version of the Lawson Diagram used at the 1971 hearings is shown in Fig. 1; a modern enlargement from this diagram is given in Fig. 2. The solid circles illustrate the very strong continuing progress in the experimental performance of the tokamak. Figure 2 includes our projections for the newly completed Tokamak Fusion Test Reactor (TFTR) and the proposed Tokamak Fusion Core Experiment (TFCX). Theory has also made a contribution to the Lawson Diagram since 1971: The "unthermalized break-even" curve in Fig. 2 illustrates a marked relaxation of requirements when energetic ions are used to heat the plasma.

To say that we had a crystal ball in 1971 would not be quite accurate. Broadly speaking, we tended to overestimate the effectiveness of the concepts we were familiar with then, and to underestimate the effectiveness of new techniques, such as neutral-beam heating. These two forms of ignorance seem to have cancelled out quite well -- with an appreciable positive residue.

A more detailed picture of progress towards a tokamak reactor is given in Table I, which compares my 1971 testimony regarding experimental status, next-step goals and reactor goals with my present testimony. Perhaps the most striking fact conveyed by this table is that our tokamaks have now succeeded in approximating -- on an individual basis -- each of the major parameters of a fully ignited toroidal reactor:

1. Neutral-beam heating in PLT has given reactor-like ion temperatures

   $T_i = 7$ keV $\sim 1/3\ (T_i)_{reactor}$

2. Initial ohmic heating in TFTR has given reactorlike energy confinement times

   $\tau_E \sim 0.3$ sec $\sim 1/3(\tau_E)_{reactor}$

3. Ohmic heating in Alcator C has given a reactorlike Lawson parameter

   $n\tau_E \sim 8 \cdot 10^{13}$ cm^{-3} sec $\sim 1/3(n\tau_E)_{reactor}$

4. Neutral-beam heating in D-III has given a reactorlike ratio of plasma pressure to magnetic field pressure

   $\beta = 4.5\% \sim (2/3)(\beta)_{reactor}$.

The TFTR -- which was mentioned only briefly in the 1971 testimony as the appropriate tokamak feasibility experiment -- is now in successful experimental operation. I can't resist showing you the initial data on energy confinement scaling in the TFTR ohmic-heating phase (Fig. 3). The results are at the optimistic upper edge of our prior expectations, and suggest that large-tokamak research may turn out to be an encouraging as well as an enlightening activity. It is important to keep in mind, however, that the most critical experiments for assessing the quality of tokamak confinement in the reactor plasma regime are yet to come.

This summer, TFTR will begin to use neutral beams to push the plasma to high ion temperatures. The TFTR plan is to enter the break-even range in 1986. During the three-year period '84, '85, '86, the combined efforts of the TFTR, the Joint European Torus (JET) and smaller supporting tokamaks should complete the required data base for the prediction of detailed ignition requirements in a tokamak reactor.

The U.S. program strategy is to take timely advantage of this information flow by preparing for the experimental realization of an ignited, long-burning reactor core -- the TFCX. A national TFCX design team, headed by PPPL, has been assembled and a number of promising design options are currently under study. Interest has concentrated primarily on tokamak plasmas with currents in the range of 7-12 MA, fusion powers of 200-400 MW, burn times of 100-300 sec.

In the course of the present year, the TFCX preconceptual design choice will be narrowed down, and industrial participation in the TFCX design team will begin. By '86, a TFCX conceptual design will have been iterated in the light of experimental tokamak studies and other R&D activities. If preliminary design of the TFCX is undertaken in '86 and construction in '87, then

TFCX experimental operation could begin in late '92. (The proposed TFCX schedule has turned out to be displaced almost exactly one decade from that of TFTR, which entered preconceptual design in the spring of 1974.)

What predictions can be made for the magnetic fusion program during the next twelve years? Since we still don't have a crystal ball, my prediction is that we will follow the same pattern as during the past twelve years: The concepts that we think are well in hand will turn out to require revision; unexpectedly successful new developments will help us out; the most likely net effect is that we will continue to meet our objectives.

There is a particular point here that deserves amplification: New developments are seldom really "new," in the sense that their success comes as a total surprise. Neutral-beam heating, for example, was already mentioned in the 1971 testimony, as were the advantages of high magnetic fields and special plasma shaping. Similarly, there is now a whole new generation of innovative ideas in such areas as plasma shaping, rf-heating~~,~~ rf-current-drive, fuel pellet injection and plasma impurity control. Their real value will be proved only in the course of the next several years of experimental work -- but some of them clearly have the potential to change the looks of tokamak reactors.

The same sort of comment can be made for the trend of fusion research in general. The success of the tokamak is not likely to be a physically isolated phenomenon. Better understanding of how the tokamak reactor plasma works may help lead us forward to radically simpler and cheaper reactor systems. Again, we should not expect new concepts to come out of the blue: At the heart of a successful fusion program there must be an intensive, discriminating search for seminal ideas, followed up by realistic development plans and schedules.

Turning to the topic of future support for fusion research, I would therefore make the following principal recommendations: To test the reality of fusion power, the program needs to press on towards a burning reactor core. Accordingly, the TFCX must be the priority program effort. To insure the experimental success of the TFCX and to prepare for the practical opportunities that may follow on the achievement of ignition, it is also absolutely essential to support the development of seminal ideas, both in the tokamak concept area itself and in other lines of approach to a magnetic fusion reactor.

The Department of Energy's proposed fusion program for FY'85, as presented by Dr. Trivelpiece in his testimony of February 23, seems to me to implement this philosophy in a realistic way, subject to the constraints of a tight overall budget.

HPF:dvm
February 29, 1984

Table I

Key Parameters of a Tokamak Reactor

	Ion Temperature T_i (units of keV or 12 million°C)	Energy Confinement Time τ_E (milliseconds)	Lawson Parameter $n\tau_E$ Density (cm^{-3})x Time (seconds)	Ratio of Plasma to Magnetic Pressure β (%)
1971 Testimony				
Experimental Status	0.5	10	$5 \cdot 10^{11}$	0.2
PLT Goals	2.5	350	$3 \cdot 10^{13}$	1.0
Reactor Goals	10	700	$4 \cdot 10^{14}$	5.0
1984 Testimony				
Experimental Status*	7 (PLT)	300 (TFTR)	$8 \cdot 10^{13}$ (Alc C)	4.5 (D-III)
TFTR Goals	15	300	$3 \cdot 10^{13}$	2.0
TFCX Goals	20	1000	$2.5 \cdot 10^{14}$	7.0

*Individual present-day experiments have come within a factor of 3 of the key parameters of a tokamak reactor: T_i, τ_E, $n\tau_E$, and β. The neutral-beam-heating phase of TFTR is expected to have sufficiently good simultaneous parameters to enter the break-even range, and the TFCX is aimed at full reactor parameters: i.e., ignition and equilibrium burn.

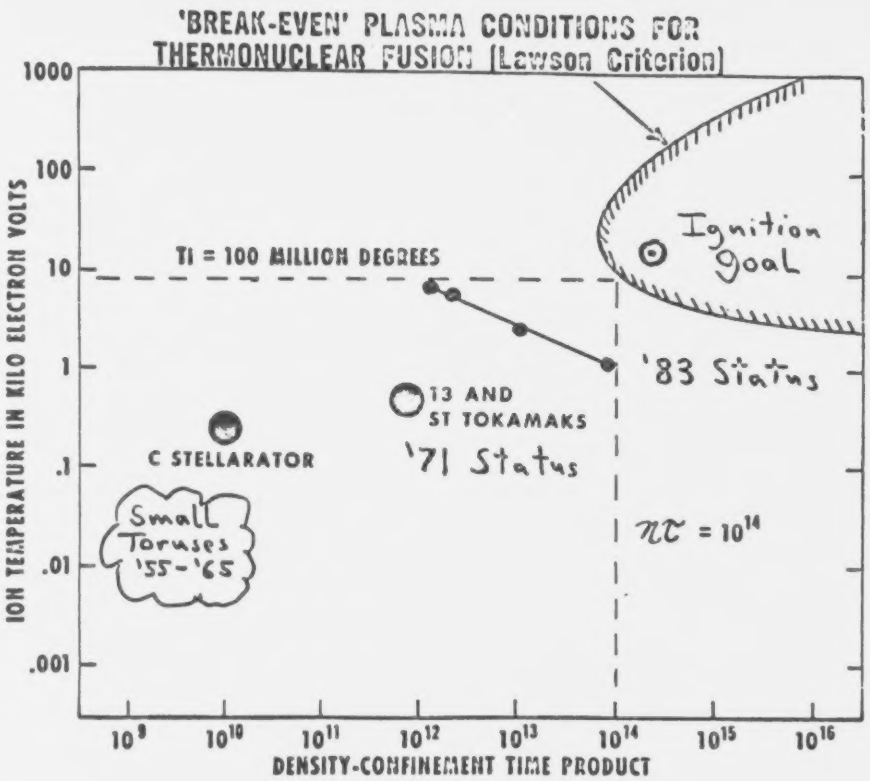

Figure 1. Edited version of a Lawson Diagram from the 1971 hearings, with present experimental results and reactor core objective superimposed.

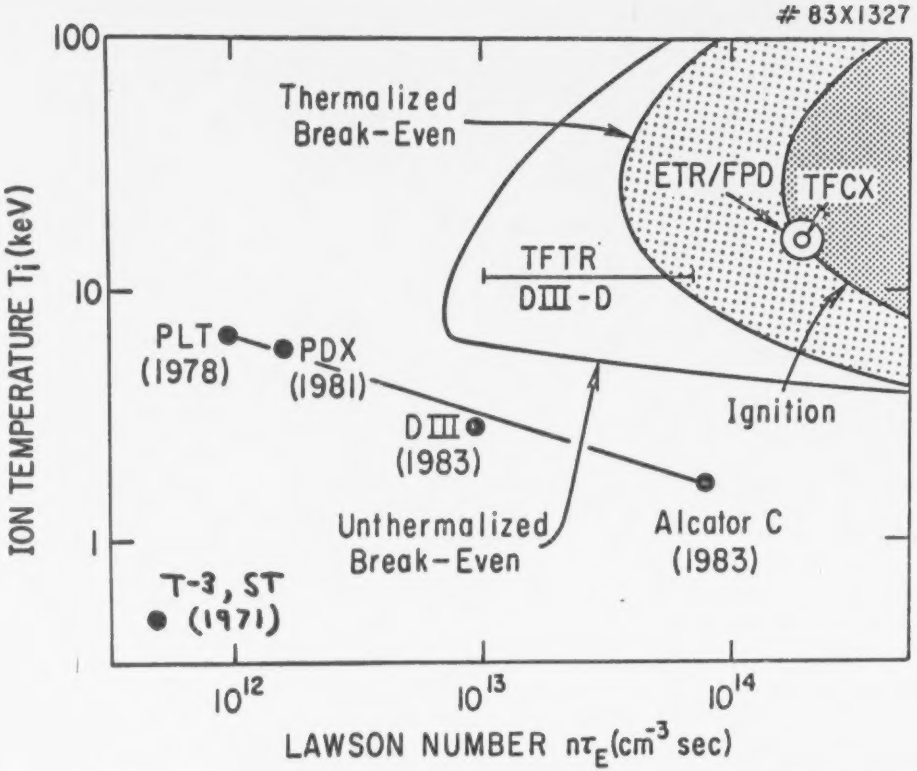

Figure 2. Enlargement from the Lawson Diagram of Fig. 1. In the TFTR neutral-beam heating phase, an unthermalized hot-ion plasma will be produced, which is expected to enter the break-even range. The TFCX will operate on the ignition curve, A future Engineering Test Reactor or Fusion Power Demonstration would operate at the same point.

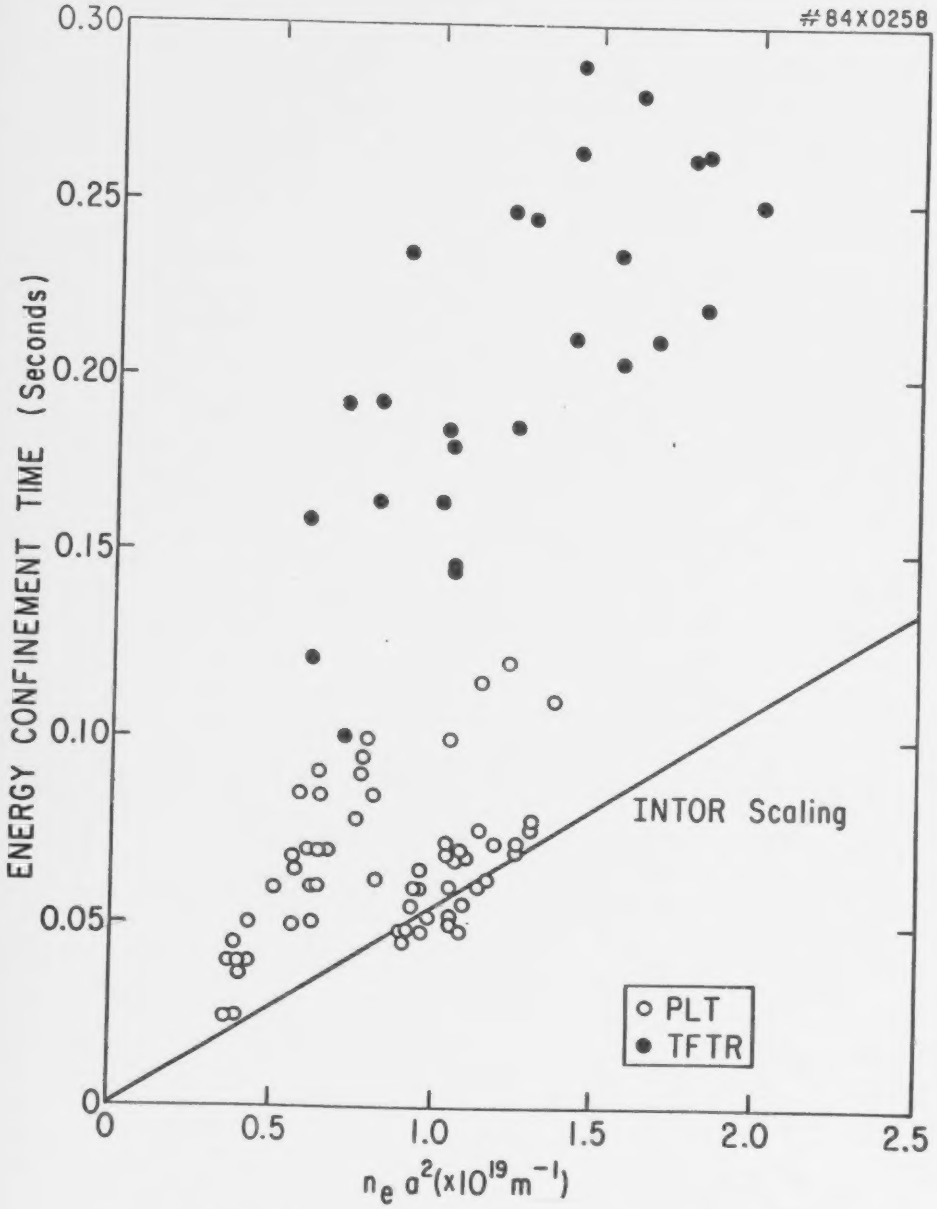

Figure 3. Initial results for energy confinement in the ohmic-heating phase of TFTR (at approximately half the maximum values of current and field). The confinement is rising in proportion to the plasma density n and the cube of the plasma size. By contrast, the scaling law used in the international tokamak reactor study INTOR assumed that confinement would increase only as the square of the size.

Mrs. LLOYD. Thank you very much. We certainly appreciate your tremendous contribution.

Dr. Ohkawa, would you please proceed with your testimony.

STATEMENT OF DR. TIHIRO OHKAWA, VICE PRESIDENT, FUSION AND ADVANCED TECHNOLOGY, GA TECHNOLOGIES INC.

Dr. OHKAWA. Madam Chairman and members of the committee, I am Tihiro Ohkawa, the vice president of GA Technologies with responsibility for magnetic fusion research and development. Thank you for inviting me to testify before you today on the fiscal year 1985 magnetic fusion energy budget. More importantly, thank you for your support of the fusion program in general and of GA in particular.

GA Technologies has the broadest involvement in fusion research of any industrial concern in the world. We have been a contributor in the three decade-long progression of the fusion program starting from primitive benchtop experiments to sophisticated devices utilizing advanced technology and approaching plasma conditions of a reactor. Based on the experience gained from participating in this progression, I wish to state my personal view of the proper direction for the magnetic fusion program.

The top priority is an unequivocal demonstration that fusion is here. They key to such a demonstration is producing a fusion fire, an ignited plasma. The tokamak program has established the scientific basis to proceed with construction of such a device, the TFCX. In parallel with TFCX, we must vigorously pursue a variety of confinement approaches, including the tokamak, that hold the most promise for attractive reactor embodiments. This strategy of pushing ahead with a demonstration of fusion in concert with evolving the reactor concept is in accord with the recommendation of the Energy Research Advisory Board and also the Magnetic Fusion Advisory Committee.

The pace of progress in magnetic fusion research has continued to accelerate. In the tokamak area, the past 18 months has witnessed a number of important programmatic and conceptual milestones, most notably, as already mentioned by Dr. Furth, achievement of good confinement sufficient for ignition; demonstration of the acceptable power densities, demonstration of beta; production of plasma current via radio frequency techniques alone, the current drive; and favorable confinement trends in larger devices, shown by TFTR and JET.

These exciting advances have served to crystallize community support for proceeding with TFCX. Indeed, all that we have learned in recent years has increased our confidence level in the ability to design and execute an ignition-level project. Moreover, the plans for existing devices in the coming 12 to 24 months will provide natural checkpoints for determining the design specifics. The GA program will play an important role in providing this design input. Our principal research vehicle, the Doublet III program, will be upgraded through the installation of a large dee vacuum vessel that provides an ideal test bed for simulation of TFCX.

In fact, the GA program in many respects reflects the direction the national program is taking. Our largest program element is Doublet III, a tokamak that forms an integral part of the preparation for TFCX. But we also have a major involvement in alternate concept research through the OHTE program. I am happy to report that recent experimental results have shown that a small, ohmically heated reactor may indeed be possible. While this approach is much less developed than the tokamak, its promise is so great that major corporations have chosen to support this venture entirely with private funding. Such a commitment, which is only possible because of the progress being made in parallel via Federal funding, is indicative of the richness of opportunity in the quest for fusion power.

The OHTE is one example of the innovation that has characterized the history of fusion research. The innovations of the past decade, plasma shaping, current drive, methods of controlling the plasma edge, form the basis for TFCX design. I fully expect that the new ideas spawned from the basic research underway now throughout the community will dominate our concept of a fusion reactor a decade hence.

My belief is that the only way to fully assess fusion's prospects is to build a fusion engine and see how it performs. Because of the progress in tokamak research, we know enough now to make TFCX a success. In conjunction with a vigorous program to search for the optimal reactor embodiment, TFCX will in turn teach us enough to structure the transition of fusion to the commercial stage. I urge you to provide us with the support we need to build upon our successes and to proceed with the TFCX.

In closing, I wish to add a few remarks on the subject of international cooperation. The Japanese Government entered into an agreement for joint research on the Doublet III facility. This is one of the largest cooperative scientific endeavors the United States has yet embarked upon. Its success is owning to two perceptions in the negotiation stage: First, that each side would benefit and second, that the program in question was a firm commitment of the host government. I conclude that while international cooperation would certainly be advantageous for TFCX, it will only happen if it is clear that the project will be undertaken regardless of the outcome of the negotiation process. Hence, we should explore the cooperation route in parallel with getting the project underway.

Your continued strong support of the magnetic fusion program is greatly appreciated.

That concludes my remarks. Thank you.

[The prepared statement of Dr. Ohkawa follows:]

TESTIMONY BEFORE THE

HOUSE SCIENCE AND TECHNOLOGY COMMITTEE
ENERGY RESEARCH AND PRODUCTION SUBCOMMITTEE

on the

FY 1985 MAGNETIC FUSION ENERGY BUDGET

by

Dr. Tihiro Ohkawa
Vice President, Fusion and Advanced Technology
GA Technologies Inc.

February 29, 1984

Madam Chairman, Members of the Committee, I am Tihiro Ohkawa, Vice President of GA Technologies, with responsibility for magnetic fusion research and development. Thank you for inviting me to testify before you today on the Fiscal Year 1985 Magnetic Fusion Energy budget. More importantly, thank you for your support of the fusion program in general and of GA in particular.

GA Technologies has the broadest involvement in fusion research of any industrial concern in the world. We have been a contributor in the three decade-long progression of the fusion program from primitive benchtop experiments to sophisticated devices utilizing advanced technology and approaching reactor plasma conditions. Based on the wisdom gained from participating in this progression, I wish to state my personal view of the proper direction for the magnetic fusion program.

The top priority is an unequivocal demonstration that fusion is here. The key to such a demonstration is producing a fusion fire, an ingnited plasma. The tokamak program has established the scientific basis to proceed with construction of such a device - the Tokamak Fusion Core Experiment (TFCX). In parallel with TFCX, we must vigorously pursue a variety of confinement approaches (including the tokamak) that hold the most promise for attractive reactor embodiments. This strategy of pushing ahead with a demonstration of fusion in concert with evolving the reactor concept is in complete accord with the recommendations of the Energy Research Advisory Board (ERAB) and the Magnetic Fusion Advisory Committee (MFAC).

The pace of progress in magnetic fusion research has continued to accelerate. In the tokamak area, the past 18 months has witnessed a number of important programmatic and conceptual milestones, most notably

1. Achievement of quality of confinement sufficient for ignition.
2. Demonstration (at GA) that acceptable power densities are attainable.
3. Production of plasma current via radio frequency techniques alone.
4. Favorable confinement trends in early results from the new generation of very large tokamaks.

These exciting advances have served to crystallize community support for proceeding with TFCX. Indeed, all that we have learned in recent years has increased our confidence level in the ability to design and execute an ignition-level project. Moreover, the plans for existing devices in the coming 12-24 months will provide natural checkpoints for determining the design specifics. The GA program will play an important role in providing this design input. Our principal vehicle, the Doublet III tokamak, will be upgraded through the installation of a large dee vacuum chamber that provides an ideal test bed for simulation of TFCX.

In fact, the GA program in many respects reflects the direction the national program is taking. Our largest program element is Doublet III, a tokamak that forms an integral part of the preparation for TFCX. But we also have a major involvement in alternate concept research through the OHTE program, another confinement scheme that has had major recent laboratory successes. While this approach is much less developed than the tokamak, it is so replete with promise that major corporations have chosen to support this venture entirely with private funding. Such a commitment, which is only possible because of the progress being made in parallel via federal funding, is indicative of the richness of opportunity in the quest for fusion power.

OHTE is one example of the innovation that has characterized the history of fusion research. The innovations of the past decade - plasma shaping, current drive, methods of controlling the plasma edge - form the basis for TFCX design. I fully expect that the new ideas spawned from the basic research underway now throughout the community will dominate our concept of a fusion reactor a decade hence.

My belief is that the only way to fully assess fusion's prospects is to build a fusion engine and see how it performs. Because of the progress in tokamak research, we know enough now to make TFCX a success. In conjunction with a vigorous program to search for the optimal reactor embodiment, TFCX will in turn teach us enough to structure the transition of fusion to the commercial stage. I urge you to provide us the support we need to build upon our successes and to proceed with the TFCX.

Budget guidance for FY 1985 and beyond does not provide the enhanced funding that we would prefer to see as an accompaniment to proceeding with TFCX. Carrying out the TFCX project within a level budget will cause some erosion of the base program needed to maintain the long term schedule for fusion power.

Mobilizing the human, fiscal, and hardware resources of the fusion program to efficiently support the TFCX initiative will take time. I wish to request a $10M increment to the Congressional Budget submitted from the Department of Energy to facilitate this transition period. Of these funds, $5M will be ued to support the industrial teams participating in the design effort. Speaking as a representative of a number of industrial concerns actively involved in fusion R&D, I wish to convey how anxious we are to participate in the formative stages of the project. The remaining $5M will be applied toward research and development in the long lead time component development areas, specifically auxiliary heating and pellet injection. Initiating the component development work now will permit proof-testing these concepts on existing devices.

In closing, I wish to add a few remarks on the subject of international cooperation. The Japanese government entered into an agreement for joint research on the Doublet III facility. This is the largest cooperative scientific endeavor the US has yet embarked upon. Its success is owing to two perceptions in the negotiation stage: first, that each side would benefit and second, that the program in question was a firm commitment of the host government. I conclude that while international cooperation would certainly be advantageous for TFCX, it will only happen if it is clear that the project will be undertaken regardless of the outcome of the negotiation process. Hence, we should explore the cooperation route in parallel with getting the project underway.

Your continued strong support of the magnetic fusion program is greatly appreciated. I'd be happy to answer any questions you may have.

Mrs. LLOYD. Thank you, Dr. Ohkawa.
Dr. Baker?

STATEMENT OF DR. CHARLES C. BAKER, FUSION POWER PROGRAM DIRECTOR, ARGONNE NATIONAL LABORATORY

Dr. BAKER. Madam Chairman, I am indeed pleased to appear before your subcommittee today. I come before you as a spokesman for reactor design and reactor technology, so I would like to mainly make my remarks in these areas.

First, I want to state my very strong support for the proposed next step in the tokamak program, that is, the TFCX. The emphasis on ignition and long pulse lengths are appropriate goals. I would, of course, like to see an ambitious reactor technology mission for the device. However, we all recognize that TFCX must not have too high a cost, and thus many aspects related to reactor technology may have to be deferred. I still support proceeding with TFCX because I believe it is vital to maintaining the momentum of the program. It will be important, however, to maintain a balanced program to allow us to carry out complementary research for the most critical reactor technology issues.

In the first viewgraph is shown schematically the major progress made in reactor design over the last decade, particularly with respect to the tokamak. Ten years ago, the reactor projections were very large, partly because the projected Beta's at that point in time were quite low, only 1 percent. However, based on the results of the more recent Tokamak research programs, for example in machines like Doublet III just referred to by Dr. Ohkawa, we now can project Beta's in the range of 5 to 8 percent. This has resulted in reducing the volume of our projected reactors by almost an order of magnitude and increasing the wall loading from typically 1 MW/m2 of 10 years ago to now at least 4 to 5 MW/m2.

More importantly, you will see that at least the physical size of projected reactors are not much different than the devices we now have in construction or are projecting, such as TFCX.

In the next viewgraph is shown a study of a commercial tokamak reactor carried out by a collaboration between Argonne, McDonald-Douglas, GA Technologies, and Parsons. This looked at a number of new ideas, for example, operating a tokamak in a steady state or at least long pulse mode, which are now being explored in experimental programs with encouraging results. This has resulted, for example, in substantial reductions in materials problems with respect to fatigue considerations and other reactor simplifications as well. Improved ideas in impurity control, such as pumped limiters and simplified poloidal divertors, have further improved the tokamak configuration. We can now develop designs, for example, where the first wall and blanket can be removed and repaired without moving any magnets, a substantial change from earlier designs.

In addition, a better understanding of the materials issues have given us increased confidence that the wall loading and power density can be increased. We now believe that materials such as stainless steel will last at least 3 to 5 years in the fusion environment. This may be adequate, but more advanced materials such as ferritic steels and vanadium hold out the real promise of longer lifetimes, higher wall loads, increased efficiency, and perhaps most importantly, substantially lower radioactivity.

We recognize, however, that we must continue to improve these reactor concepts, and in particular the tokamak so that it can truly be an attractive reactor for the future. Fortunately, there are a number of ideas which maintain our expectations for further improvement. Examples include the possibility of substantially higher Beta's such the size of the device can be further reduced which will of course reduce its cost. It is probably possible that we can reduce the magnetic field levels which would further result in simplifications and lower cost and may even allow us to consider the use of copper magnets in addition to superconducting coils.

I would now like to turn to make a few comments about the general area of reactor technology. Clearly, many of these issues are very long term. It is therefore relevant to ask, why should research proceed at this point in time? I have summarized here what I think are the most important reasons.

First and foremost, it is important to note that the development of a viable first wall and blanket for any fusion device is unresolved feasibility issue in its own right and is simply not an engineering problem. It involves state of the art science and technology in key areas such as materials, chemistry, atomic physics, and the like.

Second, the selection of the first wall and blanket can, itself, significantly impact plasma engineering issues, and can also impact the suitable selection of the confinement concept itself.

Third, any intermediate device which will use a substantial amount of tritium will have to breed its own tritium. Therefore, some blanket development must proceed.

Fourth, and this is an important point, the perception of fusion's safety and environmental features is largely determined by blanket and materials technology considerations. Decisionmakers in the Congress and the public can be expected to ask more and more questions about fusion's safety concerns. We simply need more research in these areas to provide adequate answers.

Fifth, the performance of the device in terms of its nuclear systems will of course have a substantial impact on economics.

Finally, we have learned the lessons from other technologies that the development of advanced materials and supporting technologies is, indeed, a very long process, and it is none too early to proceed.

While the development of reactor technology and materials is in its early stages, there have been, nevertheless, some important results. For example, experiments this past year done in collaboration between Argonne and the Oak Ridge National Laboratory demonstrated tritium release from a miniature little capsule which was a close model of what a fusion blanket would look like. It was irradiated in the Oak Ridge reactor under conditions which would closely simulate fusion conditions.

This experiment demonstrated that the tritium inventory in these types of blankets would be at least 100 times less than envisioned before the experiments, something that we could not predict with the theoretical models but we only discovered through this type of experiment. In addition, this last year has seen several important facilities come into operation such as the Tritium System Test Assembly at Los Alamos, the 2-beam line operation of the Rotating Target Neutron Source at Livermore, the Felix Fusion Electromagnetic Induction Experiment at Argonne, and the first wall heating simulation experiment, or ASURF, at Westinghouse.

To one who has worked in this field for well over a decade, it gives me a great deal of pleasure to look forward to the near future where, for once, we will attack some of these issues with actual experiments rather than just paper studies, as has been our only weapon during the last decade.

It is very important for the U.S. program to maintain a balance between plasma confinement systems and reactor technology. In the near term, the reactor technology program should continue to focus, as it has been, on critical feasibility issues. During the rest of this decade, this will make use of existing and possible upgrades of test stands which, at least, partially simulate the important elements of a fusion environment and which will pave the way for actual integrated testing of fusion nuclear components and test facilities in the 1990's. The near term critical issues that need to be addressed include basic materials data on advanced alloys, tritium breeding and release in solid ceramic blankets, and development of an experimental data base on liquid metal corrosion and liquid metal magnetohydrodynamics.

In recent years, there has been substantial improvement in our ideas about the tokamak. We have not by any means exhausted these ideas, but have merely gotten started down the road to developing an attractive concept. We have developed a detailed understanding of the critical reactor technology problems and have put in place a number of programs to begin to address these issues. The near term vitality and the long range success of the magnetic fusion program require that we maintain a balanced approach which includes serious attention to these major issues.

I note with at least a little concern that the fiscal year 1985 budget in these areas actually is decreased somewhat. I am concerned about what this means for the future. At the current time, the DOE budget that addresses the materials and blanket area accounts for only 5 percent of the total budget. I would recommend that that effort not be decreased and, as a minimum, at least be held at the same level of effort in the future years.

Mrs. LLOYD. Do you think that if it was held at that level it would be acceptable?

Dr. BAKER. Yes. The 1985 budget shows a little decrease in that area. The decrease itself isn't significant, but it would be if we continued that trend in the future.

Madam Chairman, that concludes my remarks. I would be happy to answer any questions.

[The prepared statement of Dr. Baker follows:]

Testimony of

DR. CHARLES C. BAKER
Fusion Power Program Director
Argonne National Laboratory

Before the

House Committee on Science and Technology
Subcommittee on Energy Research and Production
Hearings on
FY 1985 DOE Authorization Request for the
Magnetic Fusion Program

February 29, 1984

Madam Chairman and members of the subcommittee, I am pleased to appear today to comment on the magnetic fusion program. I am Charles Baker, Director of the Fusion Power Program at Argonne National Laboratory. Argonne's primary role in fusion research is the development of reactor technology and reactor design studies.

First, I want to state my strong support for the proposed next step in the tokamak reactor program, that is, the Tokamak Fusion Core Experiment (TFCX). The TFCX emphasis on ignition and long pulse lengths are appropriate goals. I would of course like to see an ambitious reactor technology mission for the device as well. However, we all recognize that the TFCX must not have too high a cost and thus many aspects related to reactor technology may have to be deferred. I still support proceeding with the TFCX because I believe it is vital to maintaining the forward momentum of the program. It will be important though to develop a balanced program to carry out complementary research to resolve the most critical reactor technology issues.

The last five years have seen important improvements of the tokamak concept as it might be as a commercial reactor. In the early reactor studies carried out a decade ago the devices were low β, that is about one percent, were very large and had relatively low power density. However, based on results of recent tokamak research programs we believe that plasma β's of at least 5 - 8% can be achieved. This has resulted in reducing the volume of tokamak reactors by almost an order of magnitude and increasing the first wall loading from 1 MW/m^2 up to 4 to 5 MW/m^2.

Ideas for obtaining very long pulses or even steady-state operation have been developed and are now being explored in experimental programs with encouraging results. This has resulted in major reductions in materials problems related to fatigue and has produced a number of other reactor simplifications. Improved ideas in impurity control, for example pumped limiters and simplified poloidal divertors, have further improved the tokamak configuration. Additional developments, which have improved maintenance and reliability features, include modular blankets which can be removed and replaced without moving any magnets and the use of radio frequency heating in place of bulky neutral beam injectors.

A better understanding of the materials issues has given increased confidence that the wall load and power density can be increased. We now believe that materials such as stainless steel will last at least 3 - 5 years in the fusion environment. This lifetime may be adequate but more advanced materials such as ferritic steels and vanadium hold out a real promise of longer lifetimes, higher wall loads, increased efficiency, and substantially lower radioactivity.

We recognize, however, that we must continue to improve our reactor concepts, and in particular the tokamak so that it truly can be an attractive reactor for the future. Fortunately, there are a number of new ideas which maintain our expectations for further improvement. Examples of these include the possibility of a substantially higher β in the tokamak which would result in further reductions in the size of the device and hence its cost. Possible reductions in magnetic field levels would also reduce cost, would improve and further simplify liquid metal blanket designs and may even allow the use of copper magnets in place of superconducting coils. In the materials area, increased attention is being given to low activation materials including improved forms of stainless steel alloys and the development of truly superior advanced alloys. In addition, design concepts have been developed for new and different liquid metal blanket designs with lower tritium inventories and with much reduced chemical reaction concerns.

Examples of other potential improvements include much more efficient current drive techniques and the development of methods to control the plasma edge temperature such that concerns of sputtering and erosion may be substantially reduced. In fact, based on recent detailed computer models and supporting experimental results we now believe that the sputtering problems in a divertor will be substantially reduced and the lifetimes of these systems will be very long. In addition to these improvements in the tokamak, a similar exciting process has been going on in the tandem mirror area which will be highlighted in the testimony of the next speaker.

Next I would like to comment on some aspects of reactor technology development. Clearly many of the issues related to first wall and blanket materials problems are long range in nature. It is, therefore, relevant to ask why should research be carried out now. First, it is important to note that development of a viable first wall and blanket for a fusion reactor is an unresolved feasibility issue for fusion which is not simply an engineering problem. It involves state-of-the-art science and technology research in such key areas as materials, chemistry, and atomic physics. Second, the selection of the first wall and blanket can significantly impact plasma physics and engineering issues including the selection of suitable confinement concepts. Third, any intermediate device that will use substantial amounts of tritium must have a tritium producing blanket. Fourth, the perception of fusion's safety and environment features is largely determined by blanket and materials technology considerations. Decision makers and the public can be expected to ask more and more questions about fusion's safety features. We simply need more research in these areas to provide adequate answers. Fifth, the performance of nuclear systems will have a substantial impact on reactor economics. Finally, lessons from other technology programs clearly show that the time scale for the development of advanced materials and technology is long and it is none too early to start this process.

While the development of reactor technology and materials is in its early stages there have been nevertheless some important results. For example, experiments during the past year have successfully demonstrated tritium release from miniature lithium ceramic blankets under irradiation in fission reactors which closely simulate fusion conditions. The experiment, called TRIO, has shown that tritium inventories in a commercial fusion reactor blanket may be at least 100 times less than we had envisioned before these experiments. In addition, last year has seen several important facilities come into operation which include the Tritium System Test Assembly at Los Alamos, the 2-beam line operation of the Rotating Target Neutron Source at Livermore, the Fusion Electromagnetic Induction Experiment or FELIX at Argonne and the first wall heating simulation facility (ASURF) at Westinghouse.

It is very important that the U.S. program maintain a balance between plasma confinement systems and reactor technology research. In the near term the reactor technology program should continue to focus, as it has been, on critical feasibility issues. During the rest of the 1980's this will mean making use of existing and possibly some upgrades of test stands which attempt to partially simulate the important elements of the fusion environment and which will prepare the way for actual integrated testing of components in appropriate fusion test facilities in the 1990's. The near term critical issues that need to be addressed include basic materials data on advanced alloys, tritium breeding and release in solid ceramic blankets, and development of an experimental data base on liquid metal corrosion and liquid metal magnetohydrodynamics.

In conclusion, recent years have seen a substantial improvement in our ideas about the reactor embodiment of the tokamak. We have not, by any means, exhausted new ideas, but rather have a good start down the road to developing an attractive fusion reactor. We have developed a detailed understanding of the critical reactor technology problems and a number of programs have been put in place to begin to address these questions. The near term vitality and the long range success of the magnetic fusion program require that we maintain a balanced approach which includes serious attention to these major reactor technology issues.

Madam Chairman that concludes my testimony. I would be pleased to answer any questions. Thank you.

Mrs. LLOYD. Thank you, Dr. Baker.

Dr. Henning, we look forward to your testimony. Please proceed.

STATEMENT OF DR. CARL D. HENNING, ASSOCIATE PROGRAM LEADER FOR FUSION DEVELOPMENT AND TECHNOLOGY, LAWRENCE LIVERMORE LABORATORY

Dr. HENNING. Good afternoon. I am Carl Henning, head of the mirror fusion program office at Livermore, and also in charge of the development and technology program. I report to Ken Fowler with whom I am sure you are familiar. I appreciate this opportunity to speak and to cover a subject that is slightly different than the previous speakers in that they have covered tokamaks and reactor technology. I would like to cover the tandem mirror concept with some emphasis on the technological innovation which we have achieved in recent studies and some of our experimental results. I will also make brief mention of the compact Torus as another example of a high power density fusion reactor concept that might lead to more economic fusion reactors.

In the tandem concept, the primary plasma is contained in a long cylinder of rather simple geometry and simple magnetic coils. This is a high Beta regime, and we prevent the leakage of this plasma from the end of the machine by producing a high positive space charge in the end cell magnets which we usually call Yin Yang magnets. This high space charge then reflects the escaping ions back into the center cell. This concept is enhanced by heating the electrons in the end cells, principally by gyrotrons, and thus we also form what is called a thermal barrier which insulates the end cell electrons from the center cell electrons. This makes it unnecessary to heat the center cell electrons, thereby increasing the overall thermal efficiency of the reactor considerably.

The recent experiments at Livermore on the TMX experiment have demonstrated both this tandem mirror concept and the thermal barrier concept.

These various concepts then have been incorporated into what you see in front of you as the MARS reactor design. This was done principally at Lawrence Livermore Laboratory, but we had a prime contractor, TRW, who in turn had subcontractors General Dymanics, EBASCO, and Science Applications Inc., and then Grummond also helped us in the study. The University of Wisconsin also participated in the study as a major participant.

The things we like about the reactor are that the long central cell provides good maintenance access and a modular construction for relatively low cost components. These components are also very similar in size and performance to the experiment that is presently under construction at Livermore called the MFTF-B, so that we would not require much extrapolation in size or performance in order to make the step to a reactor scale. This particular reactor is sized at 1200 MW electric. It has been chosen at that level so we could compare it to a light water reactor, and for no other good reason. There are a number of sizes that are available that there were existing studies at that level and we thought that would be a good choice.

In the study we have tried very hard to exploit some of the inherent advantages of fusion energy, that being safety, low activation materials, no melt-down after heat problems as you might have on a fission reactor. Also, we have used low activation, easily disposable materials so that there would not be a major waste disposal problem.

Also, the blanket system amounts to only 1 gram of tritium, and there is a total of seven grams in the entire heat transport system. This is the result of a very low tritium solubility in lithium, lead coolant, which is a liquid metal. This liquid metal also is not flammable and so the safety aspects toward fire are significantly enhanced.

On an equivalent basis with a light water reactor, EBASCO, our architect and engineering subcontractor, had estimated the costs of this particular reactor to be about two times that of an equivalent light water reactor or what a light water reactor should be, not what it is, but what it should be. While that is a sizable difference, we are quite encouraged because of the early stage of fusion development that we can be in that range. Already, since the time of the completion of this study just 6 months ago, we have identified several factors that can greatly reduce the cost once again.

Our present facility that is under construction now is the MFTF facility which will be completed in 1986. This is at a cost of about $240 million PACE. If you include the R&D and operating budgets, that would amount to about a $400 million investment. The plasma confinement here will be competitive with the tokamak, that is, confinement times of about a second and temperatures of about 150 million degrees Centigrade. This is a significant advance in technologies since all of the magnets are superconducting and the plasma heating devices are quasi-steady state. Every feature of the MARS reactor is embodied in the MFTF-B experiment, so there is a direct line from present experiments right up to a reactor scale.

The present blanket and reactor technology needs attention in addition to plasma science. As Dr. Baker had mentioned, there are a number of tests being performed in the United States and elsewhere on fusion technologies. Most of these tend to be small component tests, but we feel by the early 1990's, these should be integrated into a large system to provide practical experience and correct possible early component failures.

The MFTF-B could be upgraded for that purpose in the 1990's at a cost of about equal to the original construction, that is, about $400 million. This could be accommodated in the present mirror program budget, if it were level by the mid-1990's, and it could also be done by 1992 with a small budget increment. The plan would be to insert in the center cell of the tandem mirror a short, 4-meter long section which would burn deuterium and tritium. It is primarily driven by neutral beams, so it is not truly ignited, but it would be simulating all of the reactor components that we might find in a full-fledged reactor and at what we call a neutron wall loading of between 1 and 2 MW/m2, which is the lower bound of the reactor range.

Let me finally note that this tandem mirror was first proposed in 1976 and is a fairly recent innovation of a very long established fusion concept. It is this process of innovation which must and will continue to reach fusion's highest potential. Even today, other new ideas are emerging, one of them being the compact Tori which could lead to small, cheap reactors. Current progress at Los Alamos on the ZT-40 has been very encouraging, and they have identified a new, what they call a dynamo effect in which the plasma creates part of its own magnetic field for confinement. Although these ideas are not as advanced as the main line concepts, we think that the progress is good and they deserve continued attention as well.

Thank you.

[The prepared statement of Dr. Henning follows:]

COMMENTS ON MAGNETIC FUSION RESEARCH*

Remarks to be Presented to the
Subcommittee on Energy Research and Production,
House Committee on Science and Technology

February 29, 1984

C. D. Henning
Lawrence Livermore National Laboratory, University of California

Thank you for this opportunity to speak to you today on a subject of potential importance to this nation's energy supply in the future.

Previous speakers have described the gratifying experimental progress in tokamak research, and our present understanding of fusion reactor technology. I will concentrate on a different reactor concept called the tandem mirror, with particular emphasis on some of the technological innovations that have emerged from our system studies of a reactor based on this concept. I will also mention briefly a quite different concept called the Compact Torus. Both the tandem mirror and the Compact Torus are characterized by a high fusion power density in the burning plasma.

In the tandem mirror concept, the main plasma confinement region is a long cylinder with simple circular magnets (a solenoid). To prevent plasma leakage out of the ends, mirror devices or "end-plugs" at each end confine hot plasmas whose positive space charge reflects ions trying to escape. The overall power efficiency is greatly improved by the addition of gyrotrons to heat the electrons in the end-plugs, a concept known as the thermal barrier. Recent experiments at Livermore have demonstrated that the thermal barrier concept works.

Our latest tandem mirror reactor design for an electric power plant is called the Mirror Advanced Reactor Study (MARS). This was a two-year study conducted by LLNL with TRW as a prime industrial partner; General Dynamics, EBASCO, and Science Applications as subcontractors; the University of Wisconsin as a university partner; and significant participation by Grumman Aerospace.

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*Work performed under the auspices of the U.S. Department of Energy by the Lawrence Livermore National Laboratory under contract number W-7405-ENG-48.

The long central cell in this design, characteristic of the tandem mirror, affords good access for maintenance and modular construction with components that are not a major extrapolation from present experimental facilities. Other potential advantages include steady-state rather than pulsed operation; a means for removing impurities and helium from the plasma; and good plant efficiency, in part due to "direct converters" that convert that portion of the fusion energy deposited in the plasma as charged particles directly into electricity. The MARS design would produce 1200 MW of electricity from 2600 MW of fusion power.

Much attention was paid to the blanket design with the aim of exploiting the potential of fusion for safety, low activation, low afterheat, and simple disposal of radioactive waste. The coolant material is a lithium-lead liquid alloy that also breeds tritium. Among its attractive features is its low tritium solubility; only 7 grams of tritium are in the entire blanket and heat transport system with only 1 gram in the blanket modules themselves. Another feature of MARS is the attention paid to disposal of activated components removed at regular maintenance intervals. Each of the MARS subsystems was analyzed to be sure that, among available materials, those selected would meet Nuclear Regulatory Commission requirements for on-site, near-surface disposal (assuming that present rules for fission would apply).

As to technical credibility, every feature of the MARS plasma confinement scheme is incorporated in the MFTF-B experiment that will begin operation by 1987. Thus, we believe we would have the necessary physics data base to begin constructing a working tandem mirror reactor in the 1990's.

As to the long-term economic outlook for such a reactor, we are encouraged by the present capital cost estimates for MARS, and a comparable tokamak design called Starfire, that run around twice that of an LWR, in direct cost. Based on what we have learned from the MARS study, we already see ways to cut costs significantly, both in the blanket and in the end plugs.

In this discussion, I have stressed the need to give attention to the blanket and other reactor technologies, in addition to plasma science and technology, in order to be able to assess the practical potential of fusion for energy applications. The U.S. has been the leader in pressing forward in this direction, but thus far, both in the U.S. and other countries, reactor technology has been approached as component development in relatively small test facilities. I believe that, by the early 1990's, these activities should be integrated in a large test facility that will provide practical experience with blankets and power-system components and a means of detecting and correcting early failures prior to the operation of a full reactor. The Mirror Fusion Test Facility (MFTF-B) now under construction at Livermore could be modified for this purpose by the early 1990's.

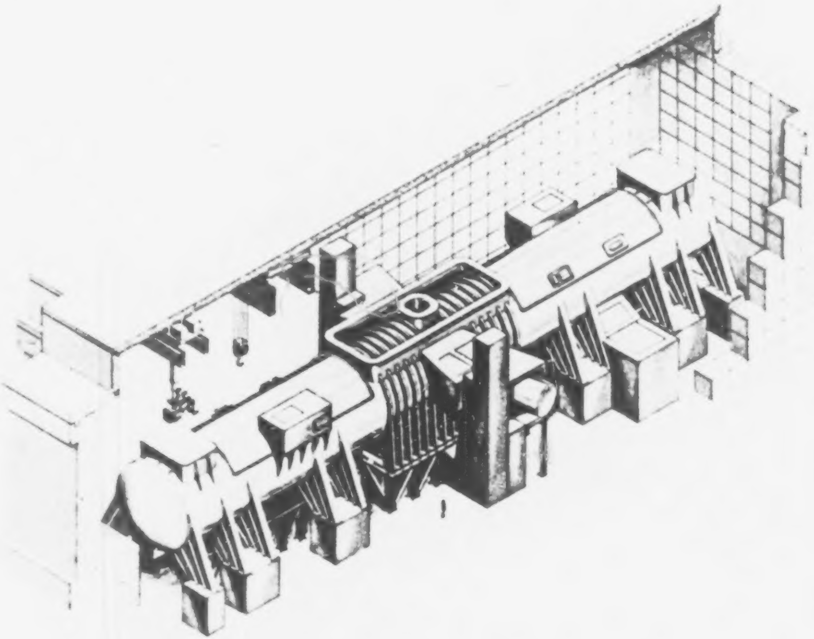
The MFTF-B itself will be completed in 1986 at a construction cost of about $240 million, or $400 million including the R&D. Its main purpose is to demonstrate plasma confinement properties competitive with tokamaks and adequate to give confidence in the tandem mirror as a viable reactor option (e.g., plasma lifetimes nearly a second and an ion temperature of 150 million degrees). Besides plasma physics, MFTF-B represents a significant advance in tandem mirror technologies. All magnets are superconducting and the plasma heating systems are quasi-d.c. (30 second pulses every 5 minutes).

To upgrade the MFTF-B for nuclear blanket testing as discussed above, we would insert into the central cell a short section, about 4 m long, that would fully simulate a reactor module including full-field superconducting magnets, blankets, and shield. Unlike MFTF-B, which will use pure deuterium plasmas, the Upgrade would operate with tritium. The plasma would not be fully ignited, but rather it would be sustained by the continuous injection of neutral beams. The cost, comparable to MFTF-B itself, could be accommodated within the present Mirror Program budget level if the project is stretched out to the mid-1990's, or, with a small budget increment, it could be completed by 1992.

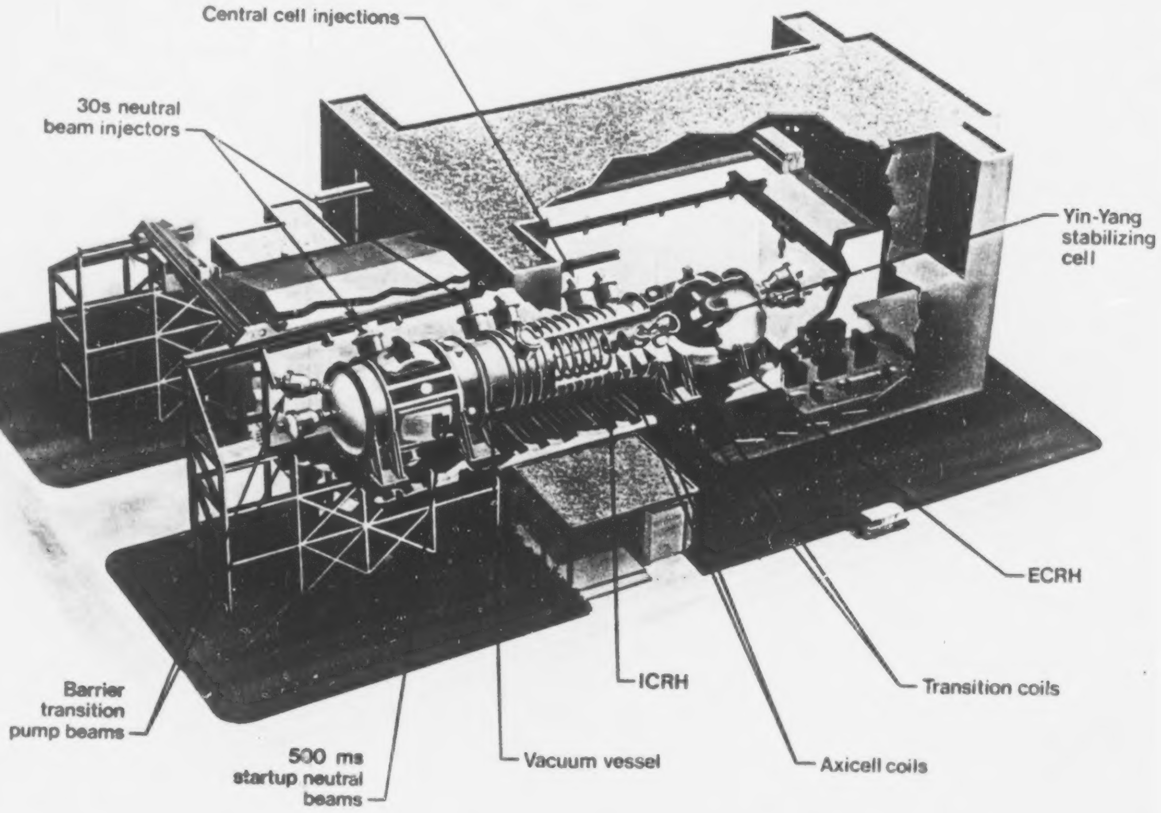
The neutron wall flux that could be achieved in MFTF-Upgrade is within the range of that anticipated in DT fusion reactors. The unique capability of tandem mirror devices to provide such test capability at moderate cost is widely recognized and may play an important role in international cooperation in magnetic fusion in the future. A study of a tandem mirror test facility called TASKA-M which is similar to the MFTF-Upgrade has recently been carried out at Karlsruhe (West Germany) in collaboration with the University of Wisconsin.

The tandem mirror, first proposed in 1976, is a fairly recent innovation in magnetic fusion research. I believe that this process of innovation, long characteristic of the U.S. fusion program, must continue if fusion is to reach its highest potential for benefit to mankind. Even today, new ideas are emerging that might change the course of magnetic fusion research in the long run. One class of such ideas well worth watching are those referred to collectively as Compact Tori. Physically, a compact torus reactor would resemble a cylindrical mirror device but one of very short length compared to the tandem mirror. In fact, a small Compact Torus reactor might be comparable in size to the MFTF-Upgrade. Experimental progress on such ideas at Los Alamos and elsewhere, though not yet as advanced as the mainline concepts, is encouraging.

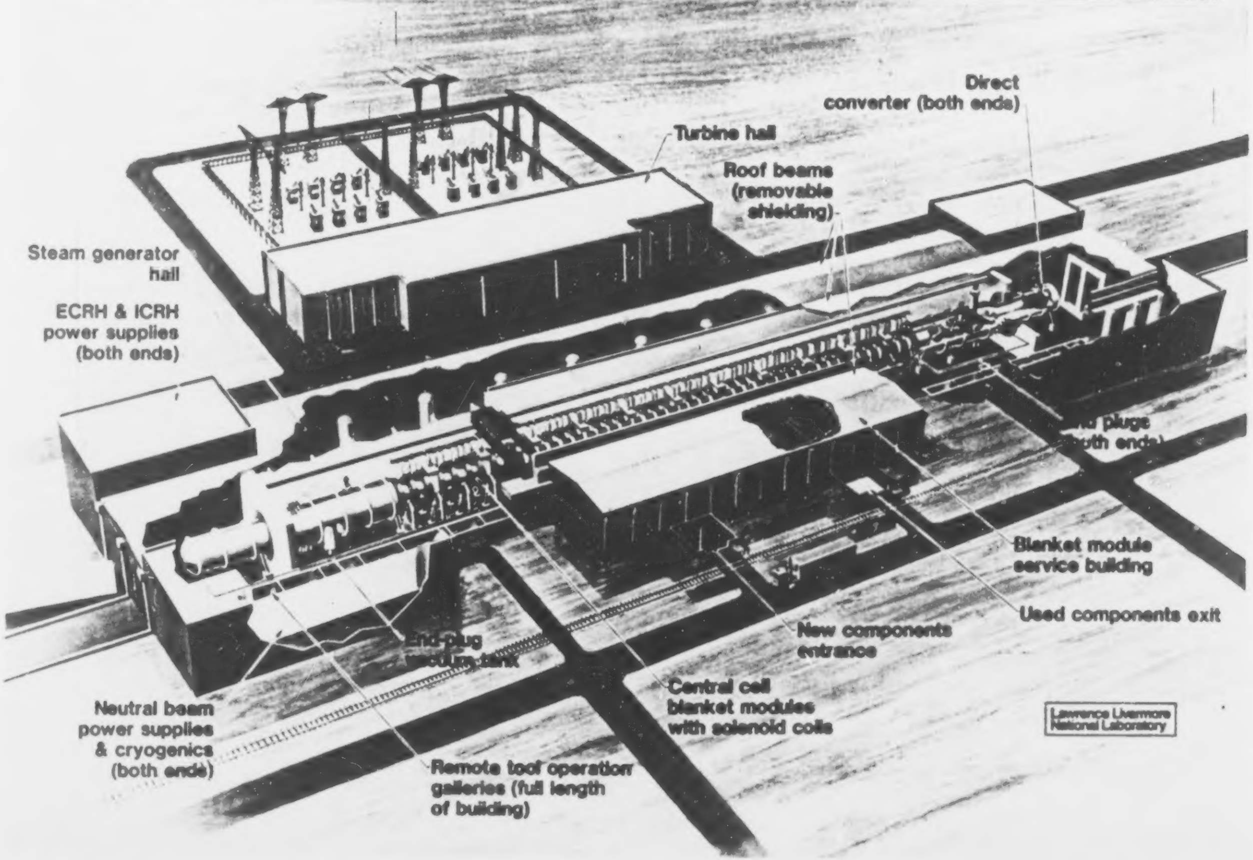
MFTF-Upgrade



THE MFTF-B AXICELL


Central cell injections
30s neutral beam injectors
Yin-Yang stabilizing cell
ECRH
Barrier transition pump beams
ICRH
Transition coils
500 ms startup neutral beams
Vacuum vessel
Axicell coils

MARS TANDEM MIRROR REACTOR


Steam generator hall
ECRH & ICRH power supplies (both ends)
Turbine hall
Roof beams (removable shielding)
Direct converter (both ends)
End plugs (both ends)
Blanket module service building
Used components exit
New components entrance
Central cell blanket modules with solenoid coils
End-plug vacuum tank
Remote tool operation galleries (full length of building)
Neutral beam power supplies & cryogenics (both ends)
Lawrence Livermore National Laboratory

Mrs. Lloyd. Thank you very much, Dr. Henning.

I would at this time like to recognize our ranking minority member of the committee, Congressman Walker, who has been a valuable part of this team. Congressman Walker, your opening statement was made a part of the record.

Mr. Walker. Thank you, Madam Chairman. I understand that the opening statement was made a part of the record at the appropriate point. I think it is more important that we get on with the witnesses than that I give the opening statement, so if we have it included, that will be good enough for me at the present time.

Thank you.

Mrs. Lloyd. Thank you very much.

Since Dr. Lidsky was asked to be a part of this panel, Dr. Lawrence Lidsky. I ask unanimous consent that his testimony be inserted as a part of the record, and I would like to read his letter to you.

I regret that a previous engagement makes it impossible to testify as requested before the Subcommittee on Energy Research and Production. I think that the subcommittee will not suffer from lack of my comments on fiscal year 1985 funding. I have not given much recent attention to detailed, short-term program planning and would not be a good witness on those issues. However, I do regret the chance to discuss somewhat longer term issues and would welcome the opportunity to discuss such matters with the committee or its staff at some future date.

My concerns with the longer term prospects of the fusion program have been somewhat overdramatized, but I have been engaged in a long discussion with my colleagues in the fusion program. We have scheduled a public forum at the Washington meeting of the American Physical Society in April.

I believe that the increasingly narrow focus on reactor relevance is premature and has damaged the scientific vitality of the program. I believe also that this premature effort to define and describe a reactor will eventually, probably sooner rather than later, damage public support for the program. I think the justification for a broad and vigorous plasma physics and fusion research program can and should be made on the basis of its possible range of application, broad intellectual domain, and demonstrated technological spinoffs.

Retreating from this lofty philosophical plane to reality, I would argue, for example, that we proceed as rapidly as possible to an ignition experiment in the most expeditious fashion, entirely indpendent of any consideration of reactor relevance. We haven't done an experiment yet in this field that hasn't held some surprise; the sooner we begin to understand ignited plasmas, the better off we will be. Then we can really begin to discuss reactor relevance.

Let me close by expressing once more my appreciation for the invitation and regret for my inability to comply.

And it is signed, Lawrence M. Lidsky, professor of nuclear engineering.

We will at this point include his testimony in the record.

[The prepared statement of Dr. Lidsky follows:]

DEPARTMENT OF NUCLEAR ENGINEERING
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
77 Massachusetts Avenue
Cambridge, Massachusetts 02139

Room 38-174
(617)253-3808

February 24, 1984

Dr. Jack Dugan
Subcommittee Staff Director
U.S. House of Representatives
Committee on Science and Technology
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Dr. Dugan:

I've enclosed a copy of a letter I have just sent to Chairman Lloyd. I'm sure you would see that letter eventually but this may cut a day or so off the elapsed time. However, my main purpose in writing directly is to enclose copies of my recent discussions with Harold Furth.

I think the exchange of views in our letters makes clear our areas of agreement and disagreement and avoids oversimplification. I hope you find it useful.

As you can tell, I feel very strongly about this issue. I've invested 20 years in the fusion program and want very much to avoid seeing the program repeating what I believe to be some of the avoidable errors of the fission program.

Sincerely,

LAWRENCE M. LIDSKY
Professor of Nuclear Engineering

LML/cal
Enclosure

DEPARTMENT OF NUCLEAR ENGINEERING
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
77 Massachusetts Avenue Cambridge, Massachusetts 02139

Room: 38-174 (617)253-3808

February 24, 1984

Representative Marilyn Lloyd
Chairperson
Subcommittee on Energy Research
& Production
U.S. House of Representatives
Committee on Science and Technology
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Representative Lloyd:

I regret that a previous engagement makes it impossible for me to testify as requested before the subcommittee on Energy Research and Production.

I think that the subcommittee will not suffer from lack of my comments on FY85 funding. I have not given much recent attention to detailed, short term program planning and would not be a good witness on those issues. However, I do regret the chance to discuss somewhat longer term issues and would welcome the opportunity to discuss such matters with the committee or its staff at some future date.

My concerns with the longer term prospects of the fusion program have been somewhat overdramatized but I have been engaged in a long discussion with my colleagues in the fusion program. We have scheduled a public forum at the Washington meeting of the American Physical Society in April.

I believe that the increasingly narrow focus on "reactor relevance" is premature and has damaged the scientific vitality of the program. I believe also that this premature effort to define and describe a reactor will eventually (probably sooner rather than later) damage public support for the program. I think the justification for a broad and vigorous plasma physics and fusion research program can and should be made on the basis of its possible range of application, broad intellectual domain, and demonstrated technological spinoffs.

(cont'd)

Retreating from this lofty philosophical plane to reality, I would argue, for example, that we proceed as rapidly as possible to an ignition experiment in the most expeditious fashion, entirely independent of any consideration of reactor relevance. We haven't done an experiment yet in this field that hasn't held some surprise; the sooner we begin to understand ignited plasmas the better off we will be. Then we can really begin to discuss reactor relevance.

Let me close by expressing once more my appreciation for the invitation and regret for my inability to comply.

Sincerely

LAWRENCE M. LIDSKY
Professor of Nuclear Engineering

LML/cal

c.c. Dr. J. Dugan

PLASMA FUSION CENTER
NOV 23 1983
RECEIVED

Princeton University
PLASMA PHYSICS LABORATORY

OFFICE OF THE DIRECTOR
JAMES FORRESTAL CAMPUS
P.O. BOX 451, PRINCETON, N.J. 08544
609-683-3555

November 18, 1983

Dr. Ronald C. Davidson, Director
Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street
Cambridge, Massachusetts 02139

Dear Ron:

I showed the Lidsky article to Peter Murray of Westinghouse, who happens to be a longtime expert on mineral extraction from seawater, as well as on fission power. He wrote the enclosed comments.

Whether or not the current prospects for extracting uranium from seawater are such as to make the further development of D-T fusion superfluous, is a technical question to which we should certainly seek to establish an answer. Lidsky's article and the enclosed remarks by Peter Murray serve to define the issues. MIT's reactor experts — particularly in the fission area — could perform a useful service for the energy community by writing a technical report on this subject.

Sincerely,

Harold P. Furth
Director

HPF:dvm

Enclosure

Uranium From Sea Water

In a recent article* on fusion power development, it was suggested that there was little need for proceeding with fusion as a long range energy option, in view of the possibility of extraction of uranium from sea-water (with the oceans containing an estimated 4.5 billion metric tons), thus providing ample supplies for fission reactors throughout the 21st Century. Because of the importance attached to such a broad assertion, the work which has been carried out on sea water uranium, both on the R&D on the process together with estimates of costs has been reviewed as follows:

- Uranium is present in sea water in very low concentrations (3 parts per billion) which in terms of volume means that 1000 tons of sea water contains about 3 grams of uranium. For comparison, the same 1000 tons of sea water contains about 26 tons of salt.

- The uranium is present as the uranyl-tricarbonate ion and early work by the British in the 1960's showed that a hydrous form of titanium oxide was the best chemical absorber of this ion. U.S., Russia and Japan subsequently confirmed this; and extensive work was carried out to determine the chemistry of the overall extraction process, to outline engineering schemes for a recovery facility and to identify likely sites for plants. (particularly at locations where movement of tide or currents could be used to create a regulated flow of sea water through extraction beds of titanium oxide).

- While the U.K., U.S. and U.S.S.R. have lost interest in this process because of the economics as detailed below, Japan has proceeded with pilot scale development of the process after obtaining 150 milligrams of uranium from laboratory studies in 1977. The pilot plant to produce 10 kilograms per year from the Seto inland sea is being built at Nio and is stated to be ready for operation at the end of 1984. The project is managed by the Metal Mining Agency of Japan with Taisei Corporation as the contractor. When the plant is completed, 1500 metric tons/hour of sea water will be led into a pool and pumped into 10 columns containing the hydrous titanium oxide absorbent. Further process steps include concentration to about 0.28% by ion exchange or ionic flotation to obtain an eluate of uranium containing 2,800 parts per million. This is then refined using the same methods as for mined uranium ore.

- Many cost estimates have been made of the process; the most recent U.S. study carried out in 1979 by Exxon Nuclear Company for DOE concluded that a plant to produce 500 metric tons of uranium oxide a year by 1995 would cost $6 billion and the product would cost $2,600 per pound (1979). The major factor is the handling and pumping of large volumes of sea water and the large land area consequently required for the plant; 20,000 acres would be needed for a 500 metric ton/year plant. The Japanese estimates are in disagreement with those of Exxon and it is claimed that extraction could be done in a commercial plant for about $250 per pound -- this is still approximately 10 times the current price of uranium. Of key importance, however, is the impact of using uranium at these price levels on the economics of nuclear power. At present levels of uranium prices, nuclear power electricity costs are about 20% lower than those from coal-fired plants. At levels in the range of $250 per pound for uranium, nuclear power would become markedly uneconomic compared with coal, and the question of obtaining large quantities of uranium from sea water becomes hypothetical.

In summary, the assertion that development of the sea water process for uranium renders fusion development unnecessary is not tenable. Extremely ingenious R&D has been carried out on the sea water process and the feasibility of the process has been demonstrated particularly by the Japanese. The costs of the process are, however, so high that they preclude its use. As the Economist (December 1st 1979) concluded, after reviewing the Japanese developments,:

> "Even so, the economics look dicey. Hitachi, like scientists in the West, is looking into more radical approaches: the use of bugs or algae to scavenge for uranium. However, this is unlikely to get beyond the laboratory for a long time -- if ever. The same goes for the use of magnetism and electrolysis.
>
> The Japanese admit that only a third-generation plant could begin to make a dent in the 2,000-3,000 tonnes of uranium oxide they now import every year. But, badly shaken by the oil crisis, they are willing to pay $12m towards an insurance policy for uranium supplies."

DEPARTMENT OF NUCLEAR ENGINEERING
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Room 38-174
(617)253- 3808

December 2, 1983

Dr. Harold Furth
Princeton Plasma Physics Lab
Princeton University
Princeton, New Jersey 08540

Dear Harold:

Do you remember when you used to want the correct answer to a question rather than the useful answer? In those days you would probably have communicated directly with the author of a paper to discuss claims you thought questionable. Not only is that an accepted mode of scientific discourse but in many cases that would save you (and other people) both time and embarrassment.

As noted in the Technology Review article, my comments regarding seawater uranium were based on recent Japanese advances in absorber chemistry. Chelating resins on fibrous substrates have been shown to have substantial advantages with respect to other absorber schemes. Apparently, Peter Murray is not yet aware of this work. The use of such chelating ion exchange resins--acrylic amidoxime, in particular--makes possible seawater filters with literally order of magnitude faster pickup rate and higher saturation loading than the old titanium oxide absorbers. The cost of recovery is, of course, a strong inverse function of both parameters.

The benefits of using ion exchange resins have been known for several years. The economic implications were discussed in detail at the recent International Meeting on Recovery of Uranium From Seawater held 17-19 October 1983 in Tokyo. The meeting was jointly sponsored by the IAEA and the Atomic Energy Society of Japan. Research groups from four countries presented independent estimates of the range of possible costs based on current absorber capabilities; they also noted that absorber research is still making progress. The range of price estimates (per pound of U_3O_8) is:

Japan	- $200-400
MIT	- 150-200
Germany	- 171-336
Sweden	- 60-340

If you think Peter Murray might be interested in recent developments, I suggest that he contact Professor M. Driscoll of our Department who has been working in this area for several years and who was our representative at the Tokyo conference. In any event, the IAEA will publish the complete proceedings, including discussions, within the next few months.

We could discuss the eventual cost of seawater uranium in more detail but it seems clear enough that fission will be "competitive enough" with coal in the 2030 - 2050 timeframe when land based uranium prices might reach the 200 $/lb U_3O_8 level. Note that even at 250 $/lb the true cost of electricity would only double, at most. That represents a return to historic 1950 levels. Surely we can prepare for that during the next half century or so.

Luckily, guessing the precise cost of a commodity 70 years from now isn't necessary. The salient fact is simply that uranium is essentially inexhaustible, even without the breeder reactor.

Thus we return to my main point: fusion is not necessary to anyone but fusion researchers. We must understand real world constraints if we hope eventually to develop something that the world will find useful. If, on the other hand, what we really want is to do plasma physics (a totally admirable aim) then the promise of a reactor will not be able to provide a shelter for very much longer. I note the high energy physicists don't need to promise a pion reactor to justify their program. In fact, they were smart enough not to try.

Sincerely,

LAWRENCE M. LIDSKY
Professor of Nuclear Engineering

LML/cal

c.c.:
P. Murray, Westinghouse
R.C. Davidson, Plasma Fusion Center, MIT
N. Todreas, Nucl. Eng. Dept., MIT

Princeton University
PLASMA PHYSICS LABORATORY
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609-683-3555

December 7, 1983

Dr. Lawrence M. Lidsky
Department of Nuclear Engineering
Massachusetts Institute of Technology
77 Massachusetts Avenue
Cambridge, Massachusetts 02139

Dear Larry:

In response to your letter of December 2, I would like to assure you that I am still interested in establishing correct answers to important questions. In particular, if present U.S. research policy in fusion and plasma physics needs to be restructured in some major way, that issue is certainly worthy of accurate and objective documentation.

Your Technology Review article "The Trouble with Fusion" is not a piece of technical analysis. It is a mixed collection of arguments — some right, and some wrong — presented in a spirited but one-sided way. In other words, the article is polemical in style — as, for that matter, is your letter of December 2.

Since the publicity associated with your article placed such strong emphasis on your status as a fusion insider at MIT, it seems to me rather appropriate that technical objectivity should be served by asking the nuclear engineering faculty at MIT to take a look at some of the issues you have raised. I would be very much interested, for example, in a judicious opinion on the seawater question. Is the present outlook really such that alternative nuclear-power systems have gone the way of the zeppelin, as your article suggests?

I am also just sending off to Ron Davidson a letter from Argonne concerning reactor heat transfer (a topic on which I think your article went somewhat astray). There are several other points in your article that would benefit from further consideration, and I hope to get around to them during the coming weeks.

Incidentally, my own impression concerning uranium from seawater would be that this subject should be pursued vigorously. The number of potentially acceptable long-range energy solutions is all too small. I applaud the suggestion in your letter that making categorical economic judgments half a century in advance is risky business. The reason why such judgments have so often proved mistaken in the past is that technical experts tend to think in terms of technology that is contemporary with themselves — rather than contemporary with the period of useful application.

Sincerely,

Harold P. Furth
Director

HPF:dvm

cc: P. Murray, Westinghouse
R. Davidson, MIT
N. Todreas, MIT

DEPARTMENT OF NUCLEAR ENGINEERING
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
77 Massachusetts Avenue Cambridge, Massachusetts 02139

Room: 38-174 (617)253-3808

December 20, 1983

Dr. Harold Furth
Plasma Physics Lab
Princeton University
Princeton, New Jersey 08540

Dear Harold:

I'm gratified by your letter of 7 December. I believe the U.S. program in fusion and plasma physics does need major revision--in particular, I think the overwhelming emphasis on directed reactor development is terribly damaging both intellectually and politically. I'd like to convince you of this because I think you are well placed to help effect the needed changes with minimum damage to the program or the people in it.

With regard to your suggestion that the Nuclear Engineering faculty at MIT take a look at some of the issues I have raised, I think you should be aware of the fact that both the Technology Review article and its precursors were essentially based on discussions and interactions with the Nuclear Engineering Department at MIT. Thus my conclusions regarding seawater uranium were based on my interactions with Driscoll (see enclosure from Todreas); my comments regarding the benefits of steady state operation were based on work done with Meyer, my comments on safety were based on work with Kazimi and conversations with Rasmussen, etc.

I agree that my Technology Review article is not a "technical analysis" but it is an accurate popularization of several technical articles that have been circulating for some time now and which were presented at several meetings. In fact, rather than agreeing with your comment that my article was "polemical in style", I would argue that my Technology Review article was actually somewhat understated. In fact, many of the letters I received in response to my article made precisely that point. I softened the argument for two reasons. First, my goal is to redirect the fusion program, not to destroy it. Second, I wanted to be sure that I could defend easily every point I made in my paper.

I realize that you have delegated most questions of reactor relevance to "experts" in the field. Nonetheless, may I suggest you take another look at the actual numbers in Baker's and Smith's rebuttal letter. Even if we accept their numbers at face value, we find a heat load on the first wall at least comparable to fission reactors, heat loads on collector much higher, dba rate about 5 times higher, helium production substantially higher, the blanket much more complicated, and many critical areas "only recently receiving significant attention", etc. Baker and Smith do not agree that D-T fusion suffers from "fundamentally more difficult heat transfer or substantially more severe radiation damage limits" than fission. This is an extraordinarily weak statement to support a 30 year, 30 billion dollar program, especially when it is taken from a defense of that program.

I think we have reached the last instant of time in which the present program is justifiable and from which a more general physics-based program can be recovered. I think we are still a research program, not a development program and I think we can and should justify a much broader physics-motivated effort.

Perhaps one day we should discuss this at length; the issues are complex and writing takes too long.

Sincerely,

Larry

LAWRENCE M. LIDSKY
Professor of Nuclear Engineering

LML/cal
Enclosure
c.c. Dr. R. Davidson
Dr. N. Todreas

blind cc: Rasmussen

Princeton University
PLASMA PHYSICS LABORATORY
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609-683-3555

December 19, 1983

Dr. Ronald C. Davidson, Director
Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street
Cambridge, Massachusetts 02139

Dear Ron:

Following up on my letters to you of November 18 and December 7, I would like to encourage you and your colleagues to clarify another important nuclear engineering issue raised by Lidsky in his article "The Trouble with Fusion," namely the impact of fusion reactors on weapons proliferation.

Lidsky's article states: "One of the best ways to produce material for atomic weapons would be to put common natural uranium or thorium in the blanket of a D-T reactor, where the fusion neutrons would soon transform it to weapons-grade material. And tritium, an unavoidable product of the reactor, is used in some hydrogen bombs. In the early years, research on D-T fusion was classified precisely because it would provide a ready source of material for weapons. Such a reactor would only abet the proliferation of nuclear weapons and could hardly be considered a wise power source to export to unstable governments." I enclose a letter from Carl Henning at LLNL, in which different conclusions are reported (based on discussions with Livermore personnel in the proliferation-control area).

Brookhaven has also just completed a study entitled "Long-Term Proliferation and Safeguard Issues in Future Technologies," (BNL-33457, July 15, 1983), which devotes major attention to fusion power. This report has not yet been released, but you could obtain an advance copy (as I have done) from Dr. Leslie G. Fishbone, one of the authors. The main conclusions are that: (1) hybrid reactors could create a fairly serious materials-control problem; (2) target-pellet designs for ICF reactors have a potential for dissemination of weapons-related ideas; (3) in the case of magnetic fusion reactors, the production of tritium would not in itself create a serious hazard, while "future scenarios for illegally obtaining fissile material from a fusion reactor by misuse or clandestine construction seem far less plausible than scenarios involving clandestine conventional fission reactors."

The Brookhaven study does not consider reactors using advanced fusion fuels. That topic, however, deserves at least passing attention. The D-D reaction is an excellent neutron source (not requiring any tritium for start-up), and a fusion technology that is capable of burning "neutron-free" fuels could easily handle an admixture of deuterium. The logical extension of the Brookhaven conclusions would seem to be that no pure fusion power reactor has a direct proliferation potential, while all pose an indirect hazard, if their technology is misused.

Lidsky's charge that D-T fusion power will aggravate the world's proliferation problems is rather grave — especially since he has repeated it in The Washington Post (November 13, 1983). It seems to me that MIT is likely to have some further expertise in this field and that you could do a public service by setting the record straight.

Sincerely,

Harold P. Furth
Director

HPF:dvm

Enclosure

cc: ✓Dr. L. Lidsky, MIT

Lawrence Livermore National Laboratory

December 16, 1983

Dr. Harold Furth, Director
Princeton Plasma Physics Laboratory
Princeton University
P.O. Box 451
Princeton, NJ 08540

Dear Harold:

Regarding a horizontal weapons proliferation issue for fusion, there are two major factors suggesting that fusion energy should not greatly increase the probability of nuclear weapons production. While there is little question that the 14 MeV neutrons from a first-generation fusion reactor can be used to breed fissile materials like plutonium, uranium is not a normal material in a pure electric fusion reactor. Normal decay products from activated fusion reactor components are primarily alpha, beta, and gamma emiters - - not the few MeV fission neutrons from fissile materials. Accordingly, the transport of fissile materials in and out of a reactor vault can be monitored with existing systems for fissile power reactors. We can also expect that a standard fusion reactor will have a unique radiation signature both during operation and shutdown. Periodic monitoring of the radiation at sensitive areas within the reactor vault in or near the reactor may indicate the presence of nonstandard components like fissile materials.

It would be technologically much easier and cheaper to produce plutonium from small fission reactors. The heavy water reactors fueled by natural uranium are particularly well suited for this function. A disadvantage of using power reactors with high-fuel burnup for weapons grade materials production is the production of Pu^{240} and Pu^{241}, which are undesirable in nuclear weapons. All American light water reactors are particularly non-economic in this mode because of the fuel rod expense and the six-week-long fuel change operation which dictates a large fuel burnup for economic operation and a resulting production of undesirable higher isotopes of plutonium. However, in the USSR, commercial power reactors are probably used for weapons production. This is also done in the UK, France, and the US Hanford Reactor. They use a system in which the fuel can be cycled without shutting down the reactor; because the fuel form is cheap, it can be cycled with fractional burnup and still be economical.

Finally, fuel reprocessing is a major factor. Even is fusion were used to produce plutonium, a rather costly and technically significant reprocessing plant would be necessary. The Barnwell reprocessing plant would have processed 1500 tons/year of nuclear fuel at a capital cost of about 1 B$. This is a chemical Purex process in which the uranium is dissolved in nitric acid and the plutonium and other transuranics are chemically separated. A smaller plant might be built by a small country for a few hundred million dollars with sophisticated but available technology. I understand that India has such a plant and Pakistan, Brazil and Argentina are expected to have one too. Also, it is possible for a third world country to do small batch processes to produce enough material for a small number of nuclear weapons, but this was always possible and not relevant to large material production linked to a large fusion plant, unless the fusion plant were optimized and dedicated to fissile materials production.

Now under development at LLNL, ORNL and TRW are new laser and plasma isotope separation processes. These plants are nearly an order of magnitude cheaper than previous enrichment facilities and might enable one to make weapons grade plutonium from fission reactor-spent fuel rods. If so, this further suggests that fusion is unnecessary and more complicated than other means of plutonium production.

Regards,

Carl

Carl Henning

(2165a/102a)
cc: T. K. Fowler
D. E. Baldwin
B. G. Logan
R. W. Moir
J. D. Lee
D. W. Dorn

Princeton University

PLASMA PHYSICS LABORATORY

OFFICE OF THE DIRECTOR

JAMES FORRESTAL CAMPUS
P.O. BOX 451, PRINCETON, N.J. 08544
609-683-3555

December 20, 1983

Dr. Ronald C. Davidson, Director
Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street
Cambridge, Massachusetts 02139

Dear Ron:

Following up my letters to you of November 18, December 7 and December 19 with (I hope) a final letter on the subject of the Lidsky article, I would like to ask you and your colleagues at the MIT Plasma Fusion Center to clarify the fundamental issue of "neutron-producing" vs. "neutron-free" fusion fuel that is raised in the article.

A good deal of work has been done on the potential of advanced fusion fuels by reactor-studies groups at the University of Illinois and UCLA, and by others as well, including PPPL. Proposals for neutron-free fusion plasma confinement can be categorized under three main headings: (1) the physics is fairly well understood; (2) the physics is quite uncertain; (3) the physics is counterindicative.

In the first category, an extensive study was prepared for EPRI a few years ago by TRW and UCLA, entitled "Evaluation of Proton-Based Fuels for Fusion Power Plants" (RP 1663-1). The abstract of this report states:

"The primary conclusion from our study is that, with presently known physics, no proton-based fuel appears to be sufficiently energetic to be a viable fuel for a commercial fusion reactor. The p-^{11}B cycle is very neutron-lean but does have significant plasma radioactivity. Limited hands-on maintenance of the reactor first wall may be possible 45 days after shut-down because of the low neutron level. Even with low activation materials, the neutron-induced radioactivity is not negligible. Afterheat cooling is not required. The octupole confinement concept is shown to significantly reduce one of the most important plasma energy loss mechanisms for proton-based fuels, synchrotron radiation, because of the large field-free volume and plasma reabsorption."

The TRW-UCLA report was subsequently found to be too optimistic, as discussed in the enclosed letter from one of its authors, Bob Conn. The conclusions of these researchers seem to be in marked contradiction with Lidsky's statement that p-^{11}B is "the only real hope for fusion." (On a smaller scale of subject matter, the TRW-UCLA report also contradicts Lidsky's comment that "no neutrons capable of inducing radioactivity" would be produced by a p-^{11}B reactor.)

The second category of "neutron-free" reactor concept, is illustrated by the enclosed letter from Rand McNally, a long-time advocate of advanced fuels. Leaving aside the issue of p-^{11}B energy multiplication, one may well ask whether the additional problem of low power density might not be overcome by a sufficiently powerful high-beta plasma-confinement scheme, such as that advocated by McNally. Innovative thought on this topic should clearly be encouraged — but if a consistent economic comparison is to be made between p-^{11}B and D-T fusion fuels, then it would be logical to assume a comparable degree of plasma confinement quality for both cases (see my enclosed reply to McNally). A balanced approach is taken in the enclosed letter from George Miley, who heads the effort on advanced-fuels reactor studies at Illinois.

Which reactor proposals should be placed in the third category is, of course,a matter of judgment. The "migma reactor" concept invented by Bogdan Maglich of Aneutronix — a private-sector enterprise — has been submitted to a number of review panels, who have drawn negative conclusions about its scientific content. (I understand there was a JASON review just last summer. You might be able to get some particulars from Dick Garwin.)

Lidsky's article does not make clear which of the three categories of p-^{11}B-burner he had in mind. I find it hard to believe that he was referring to the migma reactor — but I must say I am a little taken aback by the enclosed article from High Technology (June, 1983), which describes Lidsky as a "consultant to Aneutronix." The Technology Review article — and particularly The Washington Post article — are of obvious financial value to Aneutronix. If Lidsky really is their consultant, he should have identified himself as such.

Sincerely,

Harold P. Furth
Director

HPF:dvm

Enclosures (5)

cc: Dr. L. Lidsky, MIT

DEPARTMENT OF NUCLEAR ENGINEERING
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
77 Massachusetts Avenue Cambridge, Massachusetts 02139

Room: 38-174 (617)253- 3808

January 13, 1984

Dr. Harold P. Furth
Plasma Physics Laboratory
Office of the Director
Princeton University
Forrestal Campus
P.O. Box 451
Princeton, New Jersey 08544

Dear Harold:

I've enjoyed reading the responses you generated to my Technology Review article. I'm more sure now than ever that my conclusions were correct and that it was essential to raise the issue in such a way that it could no longer be ignored by those within the program.

There is somewhat less to respond to in the last two letters (12/19 on proliferation, 12/20 on neutron-free fuels) than in the first two but we should have a complete record.

With respect to proliferation, note that I spoke specifically of unstable governments: I wasn't contemplating Hell's Angels running a basement tokamak. The question is whether a D-T power reactor would be capable of generating substantial fissile material. You must, of course, have noted that Henning's second sentence concedes just that point. You might not, however, realize just how potent a source a fusion reactor is. I have just reviewed a blanket design for a 1,000 MW_e hybrid plant (only 300 MW_{th} fusion power) that would produce 1.4 tonnes/year of plutonium at nearly ten percent enrichment. 1400 kg/year! Perhaps we could do a public service by setting the record straight as you suggest but that would hardly do the prospects of the fusion program much good.

The issues raised in the letter of 12/20 need little discussion. I quote from the introduction to my article,

> "Of course, we do not know how to build a reactor to ignite such "advanced" fuels. Indeed, we know that neutron-free reactions cannot be ignited in the magnetic bottles developed for D-T and unfortunately, little of the physics painstakingly developed for D-T fusion will apply. There is no clear path for an alternative scheme and, not coincidentally, almost no support."

(cont'd)

And from the last paragraph,

> "Neutron-free fusion is a quintessential example of a high-risk, high gain area of physics that might also provide a good answer to an engineering problem. We have no guarantee that an answer exists."

The word"might" was italicized in the printed version.

With respect to your comment about identifying myself as consultant to Aneutronix, it would make far less sense for me to so identify myself than it would as a consultant to Grumman, Westinghouse, EPRI or any number of other firms or national laboratories with which my ties are a lot closer. For your personal information, I consulted (once) for Aneutronix on Maglich's request after he read the original NSF paper upon which the Technology Review Article was based. I'd guess I probably have several orders of magnitude less at stake in terms of money or personal prestige in the fate of Aneutronix than, for example, any major laboratory director might have in the maintenance of the status quo.

More to the point, however, is the fact that the status quo cannot last. As I'm sure you are aware, all the auguries indicate that the end is near. There isn't much time left to lead the program to scientifically defendable terrain. The further we go in the current direction, the harder the crash will be.

Sincerely,

LAWRENCE M. LIDSKY
Professor of Nuclear Engineering

LML/cal

c.c. Dr. R.C. Davidson

Princeton University

PLASMA PHYSICS LABORATORY **OFFICE OF THE DIRECTOR**

JAMES FORRESTAL CAMPUS
P.O. BOX 451, PRINCETON, N.J. 08544
609-683-3555

January 25, 1984

Dr. Lawrence Lidsky
Massachusetts Institute of Technology
School of Nuclear Engineering
Cambridge, Massachusetts 02139

Dear Larry:

Thank you for your letters of December 20 and January 13. I find them contentious in tone but conciliatory in substance. Perhaps this is a good time to sum up where we stand.

1. Uranium from Seawater

(A) Your article "The Trouble with Fusion" conveyed the message that the proven potential of uranium extraction from seawater is such as to make fission breeders and D-T fusion reactors economically superfluous.

(B) Peter Murray's memorandum, forwarded in my letter of November 18, and his subsequent letter of January 3 contradict this message.

(C) Your own letter of December 2 replies that uranium extraction from seawater should not be dismissed as a viable nuclear-power alternative.

(D) Conclusion: We agree.

2. Heat Transfer

(A) Your article argued that heat transfer would be an unsurmountable problem for fusion reactors, since most of the output power is released in the form of energetic neutrons.

(B) Charles Baker and Dale Smith in their letter of November 28 point out that heat-transfer problems in a fusion reactor are really caused by the fraction of output power that is not in the form of energetic neutrons, and appear to be slightly less severe than those in fission reactors.

(C) Your letter of December 20 does not defend the original statement, but draws attention to first-wall radiation damage as a critical problem.

(D) Conclusion: We agree — though I think you underrate the opportunities for technological innovation in the radiation-damage area.

3. Proliferation Potential

(A) Your article singled out D-T fusion power as threatening a significant deterioration of the proliferation-control situation.

(B) Carl Henning in his letter of December 16, and the authors of the Brookhaven study "Long-Term Proliferation and Safeguard Issues in Future Technologies" point out that a fusion economy need not entail a significant deterioration of proliferation control.

(C) Your letter of January 13 does not pursue the point at issue; it merely reasserts that a fusion reactor is technically capable of breeding fissile material.

(D) Conclusion: We agree.

4. "Neutronfree" Fusion

(A) Your article recommended that conventional fusion research should be abandoned in favor of "neutronfree" fusion.

(B) My letter of December 20 presents evidence that this is an unreasonable suggestion.

(C) Your letter of January 13 does not repeat the suggestion.

(D) Conclusion: We agree.

The central line of argument in your article was: Conventional fusion is economically less promising than fission and environmentally less attractive than "neutronless" fusion; therefore conventional fusion research should be abandoned. Our subsequent discussions have made clear that your argument would run more smoothly in the backward direction: D-T fusion is environmentally much more attractive than fission and has much greater economic potential than "neutronless" fusion; therefore conventional fusion research should be pursued exclusively. Since the same line of reasoning leads us to two opposite conclusions, I suspect, however, that neither conclusion is very sound.

Turning from matters of substance to matters of style, I would like to request that you make a conciliatory gesture in that area as well. People who engage in controversies have a responsibility to declare their relevant personal interests. In the case at hand, you should point out that you were formerly active in D-T fusion reactor engineering studies, are now interested in advanced fission reactors and are a consultant for Aneutronix. That does not give anybody the right to suggest that your technical judgments derive from your personal employment — rather than the other way around. Similarly, I would like to ask you to desist from suggesting that people who work in fusion research are motivated by self-interest rather than by a rational belief in the mission of fusion power. As the present correspondence illustrates, the fusion program has done a pretty thorough job of ferreting out and publicizing its potential troubles. It seems rather churlish to respond "If that's how tough you say things are, they must be really bad."

In your article the attribution of base motives to fusion researchers was compounded by the suggestion that these same people also have control of the level and direction of federal spending in the fusion area. In actuality, the ultimate responsibility for technical judgment rests with the Energy Research Advisory Board. An ERAB panel on fusion research has been holding public hearings for the past eight months, and is about to issue its 1984 report. If you had availed yourself of this regular channel for criticizing U.S. fusion policy, the process of dissecting your arguments and benefitting from your ideas could have run a smoother course.

A lot of what you say deserves to be taken seriously. In particular, I agree with the statement in your letter of December 20 that "we are still a research program, not a development program." That means to me, we are still looking forward to seminal discoveries and inventions. The expectation of progress is indeed well justified by the past performance of fusion research: After all, if you had published your present worry list twenty years ago, our critics would have said, "You should be so lucky as to have only peripheral problems!"

Sincerely,

Harold P. Furth
Director

HPF:dvm

cc: Dr. R.C. Davidson, MIT

DEPARTMENT OF NUCLEAR ENGINEERING
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
77 Massachusetts Avenue Cambridge, Massachusetts 02139

Room: 38-174 (617)253- 3808

February 9, 1984

Dr. Harold Furth
Princeton Plasma Physics Lab
Princeton University
Princeton, New Jersey 08540

Dear Harold:

After a brief but heartfelt note of discord this letter will be mildly conciliatory in both tone and substance. I agree with you that our conversation has come to a stationary point but I don't think we evaluate the position in quite the same way.

But before we get to those substantive issues, let me turn to what you have called matters of style and begin with a little bit of history. The *only* reason anyone knows that I have been a "consultant to Aneutronix" is that when a reporter called to ask me to comment on Maglich's claims for Migma I volunteered the fact that since my NSF article had appeared, I had consulted one-half day for Aneutronix. I further *insisted* that if the reporter quoted me in his article that my involvement with Aneutronix be mentioned. After I took such pains to maintain proper ethical standards, when I find my own statements are used to attack both my personal integrity and the validity of my arguments I take extreme umbrage. If such issues continue to be raised I have every right to demand that everyone else show they can meet the same ethical standards that I have maintained and demonstrated.

With that off my chest, let me try to summarize the situation as I see it.

1. *Uranium from Seawater*

Peter Murray and I have instituted a dialogue. We agree that either the breeder or seawater uranium and possibly both will cap uranium prices at approximately $150/lb U_3O_8 equivalent. In either case, uranium becomes an inexhaustible resource and fusion is not unique. This was a major point of my argument.

(cont'd)

2. Heat Transfer

I claimed that heat transfer would be comparatively difficult in a fusion reactor, not impossible or insurmountable because problems in that area could be traded off for problems in other areas (e.g., power density). Baker and Smith claimed that the heat transfer in a highly idealized design might be "slightly less severe". They did not mention the tradeoffs needed to achieve that value. This was so weak a response,especially because "paper reactors" always work so much better than real ones, that I saw no need to belabor the issue, or even to point out that the technical design of Starfire has as much realism as the physics assumptions. Bob Conn's article in Scientific American probably illustrates my point regarding technological issues as well as anything could.

3. Proliferation Potential

We all agree that a fusion reactor is capable of producing a prodigious amount of fissionable material. Henning states that it "need not" entail a significant deterioration of proliferation control. This is no answer at all. Plutonium itself "need not" endanger proliferation control unless, of course, people assemble it into a bomb.

4. "Neutron-free" Fusion

Note first that I did not recommend that controlled fusion research be abandoned in favor of anything, including neutron-free fusion. I explicitly stated that I had no idea how or even whether a neutron-free fusion might work. My point was (and is) that the potential of the DT fusion reactor is not adequate to justify the long, expensive program required to reach it. This does not mean we should abandon plasma physics research, fusion research, or even the near term goal of DT ignition.

(cont'd)

Page Three
February 9, 1984
Dr. Furth

I believe that the issue of "reactor relevance", especially in its increasingly narrow interpretation, has done grievous intellectual and political damage to the fusion program. We have saddled ourselves with the issue of "reactor relevance" in the hopes of attracting large, sustaining funding. The great irony is that this self-imposed constraint will not win us the rewards we hoped for. Even as we speak, the balance between basic research and development is shifting in DOE funding plans.

The landscape is littered with the ghosts of huge programs with narrow goals; I'm sure that the longer we proceed along our increasingly focussed path, the higher the probability that our program also will come to an abrupt halt. I feel a great temptation here to quote from Ozymandias but I can resist it.

I think the justification for a broad and vigorous plasma physics and fusion research program can stand on its own merits, if the case is properly and vigorously made. Just compare its breadth, possible range of utilization, immense domain, and potential applications, to that of the high energy physics program or the planetary exploration program. Public support of such scientific endeavors will carry the program into the future, not reliance on the dubious delights of the DT reactor.

Let me close by making one last point. My paper was not so wonderfully literary, the subject not so simple to grasp, nor my reputation so glittering that it would have had the effect it did unless it just happened to catalyze a growing suspicion on the part of many observers. If we continue in our present direction it won't be long before we go over the cliff.

Sincerely,

LAWRENCE M. LIDSKY
Professor of Nuclear Engineering

LML/cal

c.c. Dr. R.C. Davidson
Dr. DAN Cohn

Mrs. LLOYD. Dr. Furth, as I have just stated, Dr. Lidsky was unable to appear today, but he did send me copies of his correspondence with you regarding the fusion program. Would you like to comment on the substance of this correspondence?

Dr. FURTH. Yes, let me comment very briefly. First of all, let me say I thought that was a very good letter. I can't say that I violently disagree with anything that Dr. Lidsky said in that letter.

Second, it is a very good thing that in the fusion community there is a wide spectrum of opinion, ranging from starry-eyed idealism and belief that the dawn of fusion will come practically tomorrow to people who are very skeptical and hard-headed and who confront us day by day with the harsh realities. So, Dr. Lidsky, in his technical mode, is not at all out of the mainstream of fusion thinking.

In this correspondence which I have had with him, I think that comes out very well. I am glad that he included it in the record. I would just like to supplement it in one way. I wrote him one final very brief letter last week which he apparently had not received, and I don't think he will mind if I add it. It is written in the usual, rather playful style of this correspondence, but in substance I don't think he will find this any more objectionable than I found his letter to you. So, let me read it if I may:

> DEAR LARRY: Thank you for your letter of February 9. Every cycle of this correspondence seems to bring us a bit closer to the conventional wisdom. The world's energy agencies have been taking for granted that a fission breeder could supply essentially inexhaustible energy at tolerable prices. Likewise, they have known that both fission and fusion reactors can be used to breed plutonium. Three decades of world fusion research have not been based on a collective delusion that fusion power is either perfect or inevitable, but on a balanced appraisal of the benefits of fusion for the environment and for proliferation control versus the long-term investments needed to make it work. I was glad to learn from Ron Davidson that MIT is organizing some relevant technical discussion groups, and that you will participate.
>
> When all the loose ends in your article are trimmed away, one message seems to me to stand out clearly: The real trouble with fusion is that fission is in trouble. If the synergism of poor quality control and overregulation is driving up the cost of fission reactors, how can fusion reactors be cheap? If there are exaggerated fears about the safety of fission power, why wouldn't they be transferred to fusion? If the taxpayers think that the energy crisis is over, why should they support research on either advanced fission reactors or fusion?
>
> Your point is that we had better be worried, and of course you are right. The only reassuring factor is the latent soundness of the case for nuclear power. Fission reactors can be built and operated for a profit. Demonstrably fail-safe nuclear systems are likely to receive a fair hearing. People really do know that the world needs to develop better energy options to get through the next century.
>
> Sooner or later, public esteem for nuclear power is bound to recover. Meanwhile, there is no logic in fusion enthusiasts taking comfort from the troubles of fission, or fission enthusiasts trying to make sure that fusion is tarred with the same brush. A constructive attitude on both sides will be helpful in getting the nuclear option back on track.
>
> Sincerely,

Thank you for the opportunity to add this letter.

Mrs. LLOYD. Thank you very much, Dr. Furth.

Dr. Ohkawa, what is the status of OHTE? I believe that this is the device that Philips and GA are working on without Federal funds?

Dr. OHKAWA. Yes. We have been running the experiments for about 2½ years. The most recent result showed that what we projected at the beginning of the experiment did indeed happen. In other words, the plasma confinement and the beta parameters look very, very promising.

Mrs. LLOYD. Thank you very much.

Dr. Baker, you commented on the level of funding for budget material. In your opinion, is sufficient attention being given right now to the materials engineering work on our advanced alloys? If it is not, what would you suggest? What direction would you suggest that we move in?

Dr. BAKER. It is difficult to say at this point in fusion what is the appropriate level on these longer range questions, but it seems to me that at least 10 percent of the program of the scope and size of magnetic fusion should be addressing these longer-range issues, first, because they are major feasibility issues in their own right, and second, they will take a very long time to resolve.

At the present time, these areas of the Office of Fusion Energy budget are perhaps about 5 percent. I don't think it is reasonable for me to argue that they should be increased from 5 to 10 percent overnight, but I would definitely hope that over, say, the next 3 or 4 or 5 years that this portion of magnetic fusion research program could increase from some $25 million to the $50 million per year range.

Mrs. LLOYD. Thank you. I am trying to move along because we do have a long witness list this afternoon.

Dr. Henning, I appreciate your appraisal of our mirror program. The MFTF-B budget has been really stretched out, as you all painfully know, to reduce these annual costs. What has been the real impact of this on your schedule and how is it related to the total costs that are always involved with a stretchout.

On the proposal for the TFCX, we are talking about $1 or $2 billion. I really don't know what the amount is but it is in that range, and it is a very difficult thing to get moving on because we don't have the administration, apparently, support for it. What is the difference, the different in the goals for the TFCX and the TFTR? We are talking about the same thing that we were originally talking about for the TFTR. I imagine the final goal is burn in both of them or sustained reaction. What is the difference in the goals of the two different machines?

Dr. FURTH. I would like to answer that. The goal of TFTR was to achieve approximate breakeven. That means that as much fusion power is produced as heating power goes into the plasma. It is also called $Q=1$. Specifically, it is to produce about 30 million watts of fusion power for one second or fraction of a second with 30 million watts of neutral beam power going in. That is what we hope to achieve for the first time in approximately 1986.

That seemed like a fabulous, almost unattainable goal in 1974 when we first proposed it. What we are now talking about is something quite different, not $Q=1$, but $Q=$infinity. You put no power in, the plasma heats itself with its own reaction products, the alpha particles of the DT reaction. That is what the TFCX is meant to do. It will produce a stretchout.

Dr. HENNING. Yes, there has been a loss of something more than a year in the total schedule because of stretchouts. While it was originally projected to be completed in the end of 1985, it looks like it will be in early 1987 before the machine is really operational.

Also, there has been an escalatory increase of something in the range of $50 to $70 million in the cost of the project as a result from our early estimates to later times. there is an impact on both.

Naturally, we are quite anxious to complete this facility because it is the one that would give us the long confinement times and high energies which essentially would prove the concept and ready the tandem mirror program for the next step.

Mrs. LLOYD. One of the objectives of this committee is to keep our schedule on line as much as possible because we are very mindful of the additional costs as we get into the length, the span of the construction period, though we hope that you will continue to keep this committee informed as you continue with your progress.

Dr. HENNING. Yes. We would very much appreciate of course the projected 1985 budget, receiving that, and then we would complete in early 1987, as I had mentioned.

Mrs. LLOYD. Thank you very much.

Mr. Lujan.

Mr. LUJAN. Thank you, Madam Chairman. I have one question that has been going through my mind.

We hope to have those pulses lasting not a fraction of a second, but 1 to 5 minutes. When we have done that, we will have demonstrated the real honest-to-goodness core of the fusion reactor. It will never have to do basically anything else except to sit on precisely that same point in the Lawson diagram.

What needs to be added in intelligent nuclear engineering.

Mr. LUJAN. I know it is way above my head how that would happen, but are you saying then that you are abandoning that original goal in the TFTR for breakeven and going on to bigger and better things?

Dr. FURTH. The original plan was to divide this virtually inconceivable leap from the experiments of the early 1970's to an ignition experiment, to divide that into two small steps in series that we would know how to take. It was originally envisioned that first we would take a step to the breakeven point and enter the real reactor plasma regime but not have the very long confinement time or the very long pulse times that would ultimately be needed.

Then, when we saw how that would work out, and when we had learned the lessons of exactly what was needed to go to ignition, then we would take the second step and go to ignition.

As it has worked out, the timing is just right for that. Things seem sufficiently promising in the late 1970's and early 1980's so we were thinking, well, maybe we shouldn't even wait, but start that second step now subject to confirmation.

Mr. LUJAN. In other words, what you are saying is, I know that in 1986 we will have achieved our goal. You are making that flat statement. Therefore, we should move on with a machine that takes into consideration the fact that we did accomplish that goal and we move on from there.

Dr. FURTH. Well, we are saying something a little more complicated than that.

Mr. LUJAN. Don't make it too complicated, now. I am having trouble understanding the very——

Dr. FURTH. We now anticipate that the TFTR will indeed succeed in achieving its goal. In fact, we already anticipated that 3 years ago when this committee endorsed the Magnetic Fusion Energy Engineering Act and we were going to march on to the FED.

Due to various budgetary circumstances, we have been forced to wait. Now, we are in a position where the most critical information will start coming out this summer, some of it has come already, more will come this summer, more will come next summer, and precisely concurrent with the process of conceptual engineering design. We will have our assumptions confirmed in detail, not just in a general way.

Therefore, if we do a preliminary engineering design in 1986, and if we start construction in 1987, we will not be proceeding simply on the basis of some general belief that we are on the right track, but an extremely concrete information on exactly what it takes and exactly what is the best way to go to do an ignition experiment. Correspondingly, I remind you that the FED which was proposed in 1980 was not an ignition experiment. It said, well, Q=5, not Q=infinity, precisely because we weren't quite sure that we could guarantee ignition. However, if we start now, we will be able to be sure that we can guarantee ignition.

Mr. LUJAN. We are entering that foal into the Kentucky Derby before it has run its first race, it seems to me. You are saying, we know this horse is really going to be a winner, so we are going to put up the $10,000 fee to enter it into the Kentucky Derby, and nonrefundable filing fee to enter it in the Kentucky Derby.

Dr. FURTH. What we are asking at the moment is for conceptual design money so that concurrent with the arrival of the experimental results, some of which have already arrived and are extremely encouraging, but concurrent with that we will be able to get ready to run, if the indication is that things look good. Now, we could, of course, do nothing. We could wait not only for the very encouraging results I showed today, but hopefully the more encouraging results of next year and the year after that, and then we could say, well, this is very interesting, most encouraging. Now we know how to do an ignition experiment and we will now pull together a conceptual design organization, and in another 3 years we will be ready to make a proposal.

That is not what we are suggesting. We are suggesting that these design and organizational activities can start now, that the initial costs are extremely modest, and that it will give an opportunity for the United States to take advantage of a 3-year lead that we have gained by building TFTR rather than some other device.

Mr. LUJAN. Are we also saying that the tokamak is the way to go in fusion so, therefore, we keep moving in that direction without really examining if there are other areas that we can achieve this?

Dr. FURTH. No, we are definitely not saying that. We are saying the exact contrary, and I am sorry that you missed my earlier testimony because I had a chart which makes that very point.

What we are saying is the tokamak is the best path to get from here to the first credible embodiment of a fusion reactor which may be economically competitive, but whether it is or not will depend on imponderables like the price of uranium in the year 2020, but that the program must pursue, at the same time, and not at all diminish or lose sight of activities that are aimed toward making more advanced, more economically competitive reactors.

So, the program path is first to go for reality, namely the real burning plasma, and then to go along the path of optimization which, in my opinion, will go on forever and lead to increasingly more attractive things.

A particular example that I showed was the compact torus which could make the same power as a much larger tokamak some day when we get smart enough, when we have understood the physics of the tokamak well enough so that we can extrapolate, when we have understood the burning plasma well enough so we can extrapolate. I have strong confidence that some day we will be able to go beyond. At the moment, though, we don't know how, so we have this two-prong approach: do the real thing and also carry on with the promising future possibilities.

Mr. LUJAN. The reason I am concerned about those alternatives is, in the fission world, one of the things that we are seeing is that we had better move to smaller machines to provide that power than what we are doing right now, the 1,000 MW machine, if you want to call it that, just creates so many problems for us that maybe if we had smaller ones at each city limit or even down to that point. I keep saying that perhaps we are moving in the same direction with the fusion program, that the great big expensive machine, without putting any money really into what are the alternatives.

Dr. FURTH. What you are seeing is that until recently, a 1,000 MW electric was the going figure for fission reactors and other large plants. Therefore, our efforts to break into the market at the easiest realistic point were aimed at a 1,000 MW electric. It is not clear to everybody that that outlook will change. It is true, at the moment, there is interest in much smaller plants, but it is not clear to the experts in the field that that will really continue. There is some opinion that maybe in 10 years people will have learned how to build large plants reliably without cost overruns and that it may come back.

However, let me just say this as a general statement; whatever turns out to be the most marketable thing, obviously we will reorient our efforts to aim toward that. As has been made clear in my talk and several of the others, there exists a lot of flexibility in fusion, so one can do that. I don't think that there will every be a fusion reactor in everybody's basement, but it doesn't have to be a 1,000 MW electric plant.

Mr. LUJAN. Thank you.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Lujan.

Our next witness is Dr. Ronald C. Davidson who is chairman of the DOE Magnetic Fusion Advisory Committee. He is accompanied by Dr. Jim Kane.

STATEMENT OF DR. RONALD C. DAVIDSON, CHAIRMAN, DOE MAGNETIC FUSION ADVISORY COMMITTEE; ACCOMPANIED BY DR. JAMES KANE, DEPUTY DIRECTOR OF THE OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY

Dr. KANE. I am the Deputy Director of the Office of Energy Research in which the fusion program is located. My reason for accompanying Dr. Davidson today as a representative of the Department are twofold: to acclaim and to disclaim.

We couldn't get along without the type of advice provided us by committees such as that headed by Dr. Davidson. In this case, this particular committee has done exactly what you hoped they would do, they have comprised a committee of very diverse backgrounds and probably diverse opinions. They work very hard. They have flushed up important issues, and they have selected priorities and given us priorities in their recommendations.

Dr. Davidson will tell you the substance of this and I intend not to duscuss it at all. I just want to assure you that we take advice of committees such as this very seriously, and I can assure you that we will consider it seriously. It is my suspicion that we will adopt many of the recommendations contained in this advice.

The disclaimer is just this: advice, no matter how expert and how essential, is just one of the aspects that goes into departmental policy. Therefore, you should not consider the views or the policies that may be expressed by this committee as being those of the Department, although I strongly suspect there will be great coincidence.

Thank you.

Mrs. **Lloyd**. We shall look for your wisdom, and we will view the testimony of our witness in that vein.

Please proceed, Dr. Davidson. We do have your complete statement and you may proceed as you wish.

Dr. **Davidson**. Madam Chairman, I am pleased to have this opportunity to address this subcommittee on behalf of the Magnetic Fusion Advisory Committee, which was formed in June 1982. As you know, this advisory committee provides advice and guidance on a continuing basis to the Secretary of Energy through the Director of Energy Research on the magnetic fusion energy program. I have reported to you on two previous occasions our recommendations in several program areas, including the recommendation to initiate a Tokamak fusion core experiment, [TFCX], that would demonstrate ignition and long pulse equilibrium burn.

In a recent charge from Dr. Alvin W. Trivelpiece, Director of the Office of Energy Research, we were asked to make recommendations regarding the priority of TFCX. The Magnetic Fusion Advisory Committee examined this question in a two-day meeting on February 9 and 10 held at GA Technologies. To summarize our findings and recommendations from that meeting, let me read to you from the MFAC letter to Dr. Trivelpiece, dated February 10. This is excerpted from the letter to Dr. Trivelpiece:

The Magnetic Fusion Advisory Committee reaffirms its belief that TFCX is the necessary next major step in the U.S. magnetic fusion program, and that high priority should be given to TFCX and to the confinement and development and technology programs in support of that project. We also reaffirm the high value attached to maintaining a broad and vigorous base program in magnetic confinement research, with emphasis on advancing scientific understanding and developing optimal fusion reactor concepts. These are the two most essential tasks facing the fusion program.

At the MFAC meeting of Febrary 9, the committee heard presentations on the initial operation of TFTR and JET and noted the strongly favorable trend of the experimental results in the ohmic heating phase. The committee also reviewed the present status of TFCX preconceptual design options and plans.

As a consequence, we make the following recommendations:

One, we recommend that TFCX be given top priority in the U.S. fusion program.

Two, we recommend that DOE invite the fusion community to plan the TFCX as a national project organized around laboratory, university, and industrial teams. We believe that this approach will help assure that the project is well conceived and executed at a reasonable cost and that it will preserve in the program the valuable resources of experienced fusion scientists and engineers.

Three, we recommend that preliminary project goals and cost estimates for the TFCX national project be specified within 6 months and that MFAC be consulted during this process.

Four, the next 3 years will provide increasingly detailed confirmation of the requirements for a long pulse ignition experiment. We expect that the growing data base from contemporary tokamak experiments will continue to be used to refine the TFCX design and to develop innovative features.

Five, in order to maximize the potential of the TFCX while protecting the essential base fusion program, we strongly recommend that DOE should continue to seek incremental support for Fusion, particularly during the peak years of TFCX construction. We also recommend that foreign participation in TFCX should receive serious attention, but should not be a prerequisite for proceeding with that project.

I look forward to discussing these recommendations with you.

This letter is signed by me as chairman of the Magnetic Fusion Advisory Committee.

That concludes my remarks on behalf of the Advisory Committee. I would like to now offer some personal observations and comments.

The tokamak approach is indeed technically ready to embark on a long pulse ignition experiment, as you heard from others who have testified this afternoon. I believe that it is essential that this be accomplished within the context of a strong base program with high priority given to the confinement and development and technology activities in support of that project.

The fusion program should also emphasize a broad research base that advances scientific understanding and the development of optimal fusion reactor approaches. As I stated earlier, these are the two most important tasks facing the program. I also recommend that incremental funding be provided to accomplish these important objectives, with a gradual ramp-up support over a several year period to a level 20 percent or larger above the present level, depending on the scope and pace of TFCX. I should add that this latter recommendation is not an established policy of DOE at this time.

[The prepared statement of Dr. Davidson follows:]

Presentation[†] to

THE SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
WASHINGTON, D.C.

by

Ronald C. Davidson
Chairman, Magnetic Fusion Advisory Committee
and
Director, MIT Plasma Fusion Center
February 29, 1984

[†] Oral Testimony

Madam Chairman, I am pleased to have this opportunity to address this Subcommittee on behalf of the Magnetic Fusion Advisory Committee, which was formed in June, 1982. As you know, the Magnetic Fusion Advisory Committee provides advice and guidance on a continuing basis to the Secretary of Energy through the Director of Energy Research on the Magnetic Fusion Energy Program. I have reported to you on two previous occasions our recommendations in several program areas, including the recommendation to initiate a Tokamak Fusion Core Experiment (TFCX) that would demonstrate ignition and long-pulse equilibrium burn.

In a recent charge from Dr. Alvin W. Trivelpiece, Director, DOE Office of Energy Research, we were asked to make recommendations regarding the priority of TFCX. The Magnetic Fusion Advisory Committee examined this question in a two-day meeting on February 9 and 10 held at GA Technologies. To summarize our findings and recommendations from that meeting, let me read from the MFAC letter to Dr. Trivelpiece, dated February 10:

> "On September 28, 1983, the Magnetic Fusion Advisory Committee recommended initiation in FY86 of the Tokamak Fusion Core Experiment, a moderate-cost tokamak reactor device (less than $1B PACE) designed to achieve ignition and long-pulse equilibrium burn. At that time, it was concluded that approximately 50% of construction funding for TFCX could be obtained from within projected flat budgets for magnetic fusion, and that the remainder would require incremental funding that ramps up over a period of several years. This letter responds to your request for further advice on TFCX in light of the recent recommendations from the Energy Research Advisory Board on the high priority for a program to demonstrate ignition and long-pulse burn, even under the constraint of a constant fusion budget in real terms over the next several years.

The Magnetic Fusion Advisory Committee reaffirms its belief that TFCX is the necessary next major step in the U.S. magnetic fusion program, and that high priority should be given to TFCX and to the confinement and D & T programs in support of that project. We also reaffirm the high value attached to maintaining a broad and vigorous base program in magnetic confinement research, with emphasis on advancing scientific understanding and developing optimal fusion reactor concepts. These are the two most essential tasks facing the fusion program. Important findings and recommendations relating to these tasks are outlined in the attached MFAC Executive Summary forwarded to you on September 28, 1983, and form an integral part of this report.

At the MFAC meeting of February 9, the Committee heard presentations on the initial operation of TFTR and JET and noted the strongly favorable trend of the experimental results in the ohmic heating phase. The Committee also reviewed the present status of TFCX preconceptual design options and plans.

We make the following recommendations:

1. We recommend that the TFCX be given top priority in the U.S. fusion program.

2. We recommend that DOE invite the fusion community to plan TFCX as a National Project organized around Laboratory, University and Industrial teams. We believe that this approach will help assure that the project is well conceived and executed at a reasonable cost, and that it will preserve in the program the valuable resource of experienced fusion scientists and engineers.

3. We recommend that preliminary project goals and cost estimates for the TFCX National Project be specified within six months and that MFAC be consulted during this process. We note that the cost-effectiveness of both long- and short-pulse options is being evaluated.

4. The next three years will provide increasingly detailed confirmation of the requirements for a long-pulse ignition experiment. In particular, the first experimental results in the most relevant parameter range will become available during the period 12 to 24 months from now. We expect that the growing data base from contemporary tokamak experiments will continue to be used to refine the TFCX design and to develop innovative features.

5. In order to maximize the potential of the TFCX while protecting the essential base fusion program, we strongly recommend that DOE should continue to seek incremental support for fusion, particularly during the peak years of TFCX construction. We also recommend that foreign participation in TFCX should receive serious attention, but should not be a prerequisite for proceeding with the TFCX project.

I look forward to discussing these recommendations with you."

The letter is signed by me as Chairman of the Magnetic Fusion Advisory Committee.

That concludes my remarks on behalf of the Magnetic Fusion Advisory Committee. I would like to now offer some personal observations and comments. The tokamak approach is indeed technically ready to embark on a long-pulse ignition experiment. I believe that it is essential that this be accomplished within the context of a vigorous base program with high priority given to the confinement and development and technology activities in support of the TFCX. The fusion program should also emphasize a broad research base that advances scientific understanding and the development of optimal fusion reactor approaches. As stated earlier, these are the two most essential tasks facing the fusion program. I also recommend that incremental funding be provided to accomplish these important objectives, with a gradual ramp-up of support over a several year period to a level 20% - 40% above the pesent level, depending on the scope and pace of TFCX. I should add that this latter recommendation is not an established policy of DOE at this time.

Thank you.

MAGNETIC FUSION ADVISORY COMMITTEE

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Los Alamos National Laboratory

Peter Murray
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Marshall N. Rosenbluth
University of Texas

Alan Schriesheim
Argonne National Laboratory

Bobby J. Tharpe
General Electric

AREA OF MFAC EVALUATION	PANEL CHAIRPERSON	DOE CHARGE RECEIVED	MFAC RECOMMENDATIONS TO DOE
#1 Tandem Mirrors & Tokamaks	Dr. John Gilleland GA Technologies	June, 1982	November, 1982
#2 Alternate Confinement Concepts	Prof. Robert Conn UCLA	June, 1982	November, 1982
#3 TFTR Upgrade Options	Dr. Harold Forsen Bechtel	June, 1982	January, 1983
#4 MFTF-B Upgrade Options	Dr. John Gilleland GA Technologies	January, 1983	May, 1983
#5 Long-Term University Role	Prof. Robert Gross Columbia University	January, 1983	August, 1983
#6 Program Priorities	Prof. Ronald Davidson MIT	May, 1983	September, 1983
#7 Role of Industry	Dr. Harold Forsen Bechtel	September, 1983	Pending
#8 Program Priorities	Prof. Ronald Davidson MIT	February, 1984	February, 1984
#9 Supporting Concepts	Dr. Rulon Linford LANL	February, 1984	Pending

Mrs. LLOYD. I imagine that is the extent of the approval, maybe not approval, of the gentleman who introduced you, Dr. Davidson.

What might the initial task of the real work for the first year of TFCX include?

Dr. DAVIDSON. What is going on already at the present time at a preconceptual level is the investigation of several design options for TFCX including superconducting and copper magnet options. There is also a schedule that would involve narrowing down of these options over the next 6 to 12 months and selecting a preferred option on which you would do more detailed design.

Mrs. LLOYD. Would you comment further on the foreign participation in TFCX? You state that it should not be a prerequisite for proceeding with the project.

Dr. DAVIDSON. My own very strong feeling is that we are at the present time in a position of leadership in fusion. It is certainly true that the European Community, if you look ahead into the late 1980's, has some very impressive facilities coming on line. I believe the opportunity for us to proceed with this project as a national project is extremely important, and I think we should begin it that way. We should actively seek foreign participation in this project with the United States as the major participant, but foreign collaboration should not be a prerequisite to beginning TFCX.

Mrs. LLOYD. Another thing I wanted you to elaborate on a little bit more was long pulse ignition test experiment. What are the objectives of this experiment?

Dr. DAVIDSON. The objectives of this experiment I think were covered quite nicely by Harold Furth in his discussion.

Mrs. LLOYD. I know, but I wanted your opinion.

Dr. DAVIDSON. OK. The basic idea would be to create a plasma at high enough temperatures, high enough values of the Lawson TAU parameter such that you could actually remove the external heating to the plasma and the alpha particle heating in fact would sustain a long pulse ignited burn. Both the ignition aspect and the long pulse aspect are very important parts of the objectives that the Magnetic Fusion Advisory Committee has recommended for this experiment.

Mrs. LLOYD. Well, how about the MIT's low cost light design?

Dr. DAVIDSON. We have been looking, as part of this national design effort, looking at some copper plate magnet options for TFCX. The design option is called LITE, which stands for long pulse ignition test experiment. It makes use of a copper plate magnet design based on the Alcator approach. Because it is copper, the requirements for nuclear shielding are less severe than for a superconducting option, so the system can be somewhat more compact. The design is such that it can withstand fairly high stresses and operate at moderately high magnetic fields and density.

If I were to compare it to other options, there are a range of such options, LITE options, with major radius from 2 meters up to about 3 meters, and the 2 meter radius option, and we are in the process now of studying it in more detail in cooperation with Princeton and design center might well be in the $0.5 billion range of construction costs, if I assume about $200 million of site contribution in addition to that.

I would hasten to point out that while such a facility might meet the long pulse and the ignition objectives of TFCX, it is also, I believe, the case that it is not as prototypical of an ultimate Tokamak reactor as a superconducting device would be.

Mrs. **Lloyd**. Thank you very much.

Mr. Lujan?

Mr. **Lujan**. Thank you, Madam Chairman.

In the whole fusion business, you talk about foreign investment, Dr. Davidson, and I think that is interesting, bringing foreign investment into the TFCX. Other than the cooperative effort with General Atomics and the Doublet III, the Japanese, there is no other foreign involvement in the fusion program, or is there?

Dr. **Davidson**. There has been for several decades an ongoing international cooperative effort at much smaller levels, at the individual researcher level and also at the level of perhaps foreign visitors bringing diagnostics for special experiments.

Mr. **Lujan**. But just little on the site things?

Dr. **Davidson**. Correct.

Mr. **Lujan**. Getting back to——

Dr. **Davidson**. My colleagues in the back remind me that the large coil project at Oak Ridge National Laboratory indeed is an international effort. Three of the six coils are made by U.S. industry and three of the coils are foreign coils.

Mr. **Lujan**. Let me get back onto my favorite subject of putting all our eggs in one basket. It bothers me that what I see coming out of DOE is into the Tokamak. That is the biggest amount of money, and Dr. Furth was wanting to follow through on that a little while ago. Maybe that is the most promising and no question about it, that is the one that more work has been done on. That is the one where we have put in more money. The Tokamak funding is about twice what the mirror program is. In the alternative concepts, I guess we were looking at about $23 million for everything else.

I haven't seen in the literature before now, but I see you are talking about compact Tori now, and that is great, and that is the direction that we need to move in. However, as many of you in the past have heard me talk about what the people call the Riggatron, for years I have been trying to get a little funding just to see if that moves. All that ever happens is that is not going to work, that is not going to work. We have to go on with the Tokamak.

Now, we are saying, give us a billion or two for the TFCX. Isn't it worth the shooting of the dice for some small amount, by comparison to the big machine funding and the new machines that we are talking about, isn't it worth that to see if perhaps what they tell me sounds promising? Who is doing some compact Tori work now? What are we doing?

Dr. **Davidson**. Los Alamos, of course, is doing compact Torus research.

Mr. **Lujan**. Basic, very basic——

Dr. **Davidson**. Can I comment? You really raised two points. One is in relation to the level of support for alternate approaches to the Tokamak, and then you raised a question in relation to Riggatron which, indeed, is a Tokamak. I would simply observe that the degree of balance within the U.S. program in terms of supporting approaches other than Tokamak is unprecedented in the world. I believe that there has been considerable emphasis, merited emphasis on pursuing alternate approaches. What you have to recognize is that from the scientific point of view, at this point in time, the Tokamak approach is the most advanced and the most developed scientifically. It is ready to make this major step in long pulse ignition physics which will certainly have an impact on the development of the Tokamak line, but it will also have an impact on the development of these other concepts.

Mr. **Lujan**. Well, let me tell you exactly where I am heading with this, and I don't want to get anybody into any trouble, but you fellows are all acquainted with Bob Bossard who is a fellow that understands the kinds of things that you do, much more than I do. Years ago, he came to us and had this throwaway Tokamak, as he called it. The concept of is, I suppose, that the plasma goes around very, very fast and wears out the toroid and you replace it, and it will do the work of the Tokamak at far, far less. At one time we were talking about $50 million. He wanted something like $7 or $8 million to begin to do something with it.

I certainly am not competent to say that that will work or it won't work, but it certainly seemed to me at the time and this was about 4 or 5 years ago that we put $7 or $8 million into this thing, but the doors were closed, completely closed, to even trying it out. That is the general impression that I get.

I am not against continuing the research in the big Tokamaks. It just appears to me that the whole fusion community just holds together and says, we are going to continue this. We are going to continue in the big Tokamaks, and anyone is a little threatening to that program, we are just going to exclude.

Now, I am not trying to get Bob into any trouble, believe me, but maybe I won't because he has been excluded all the way along, so I don't know how much worse it could get for him. Bob isn't even here, and maybe he would not appreciate my speaking as I am, and as I have continued to see it for the last few years.

Dr. **Furth**. May I comment?

Mr. **Lujan**. Yes.

Dr. **Furth**. Well, let me say, first of all, that part of this massive orthodox main line Tokamak effort that you mentioned is, of course, the direct basis for all Dr. Bussard's ideas, namely the Alcator research at MIT, and that research has not been stopped. It marches on. Just last fall, it achieved a very important new goal which I mentioned. It is very successful. It is teaching us a lot of detail about how ohmic heating in high fields works.

Where do we go from there? How do we use this very impressive product? One way to use it was indicated by Mrs. Lloyd, that it opens up an avenue to cheapen the TFCX, for example, should we choose to go in that direction, at some sacrifice to what are considered to be the most relevant reactor technologies for fusion reactors in the future, but at some savings of cost. We are not at all rejecting that. That is one of the main options that is being studied as part of the TFCX design study. If funds are tight, and if technology is to be deemphasized, we will probably wind up somewhere in that direction. So, that is the first point.

The second point has to do with the question, well, exactly where is the real fusion reactor? The high field ohmic heat to ignition approach, over the years, suffered from the complaint that this cannot lead us to a practical fusion reactor, because we cannot use superconducting coils in those congested circumstances and at such high fields. Therefore, we will use more power driving the copper coils than we will ever get back from the fusion reactor.

Mr. LUJAN. And he said that you could.

Dr. FURTH. No. Dr. Bussard's contribution was to say, never you worry. We will get the Beta value up so high that nonetheless we will have a margin for making pure fusion power, and, further, we will get the neutrons out through the copper and then into the blanket and we will breed back the tritium, and this is a good way to go. He very astutely discerned that that was the crucial issue, whether one could allege that this particular approach, which is very good for physics, would also lead to a practical reactor, or whether in fact it was a dead end.

The problem Dr. Bussard had was that nobody else would believe that. Panel after panel was summoned to look at Dr. Bussard's concepts of such a fusion reactor, and panel after panel said we don't believe it.

Now, Mr. Shanny has a chance to speak and he can speak for himself, but I am not sure he believes it. What he sees is this is a very good way to make an ignited plasma. It has some applications as a neutron source, but I don't think he is going to tell you that this is a way to make a main line power producing fusion reactor.

Mr. LUJAN. OK, but if there is a guy out there who is competent—I don't suppose that anyone can say that he is not a competent scientist—and everybody disagrees with him, as you all have, but we have got $450 million into the fusion program and what is wrong with saying, all right, here is $6 million or $7 million, let's get started. Let's see what you can do. Isn't it worth it?

Dr. FURTH. Let me make a further comment. If Dr. Bussard were alone in the world, you might want to proceed in that way. However, if you would like, I could offer you a write-in from at least 100 parties who have other ideas on how to solve this problem immediately. Then it would not constitute $6 million, but $600 million, and you might then feel that it might be more sensible to channel those funds in a more organized way, along channels that there is a consensus are productive, rather than taking 100 flyers at $6 million each.

Mr. LUJAN. I wonder how many came to Queen Isabella with the theory that the Earth was round.

Dr. FURTH. Let me just amplify that—the great art is picking the guy who came to Queen Isabella. That is not done by passing it out at random or even giving it to those who speak most loudly, because some people who speak quietly and modestly sometimes have pretty good ideas.

Along those lines, I would like to say once more that there is a still, small voice speaking out for real compact Toruses which small, without having magnets wrapped around so that you can use superconducting coils and put them at a distance. There are still small voices speaking out for that, and if that works, you have really got something. Other people such as Dr. Ohkawa here who spoke out for the OHTE, a form of the reverse field pinch, also practiced at Los Alamos, and that enjoys a great deal——

Mr. **Lujan**. I don't want to get Los Alamos in trouble either, you understand.

Dr. **Furth**. They are not in trouble. That enjoys a great deal of scientific credibility too.

Mrs. **Lloyd**. If the gentleman would yield, we are going to have to move on.

Mr. **Lujan**. Yes, of course.

Mrs. **Lloyd**. Thank you very much.

Our next panel of witnesses includes Dr. Al Mense, Mr. Len Reichle——

Mr. **Lujan**. Excuse me, Madam Chairman.

Mrs. **Lloyd**. Yes.

Mr. **Lujan**. I really hate to take up so much time, but before we get into the panels, if we could, I see Mike McCormack in the audience, and I would like to hear his views while we are still on individuals rather than going into panels. If the chairman is agreeable to that, we might just take a minute or two that Mike might come up and give us his views, on the TFCX, on the small machines, that sort of thing.

Would you do that, Mike?

Mrs. **Lloyd**. I don't think that the Chair would deny our former colleague time inasmuch as he is the author of legislation that instigated this program. In all fairness to our witnesses, we are running short on time, but we do welcome you, Mike.

Please proceed.

Mr. **McCormack**. Thank you for getting me out of that mess. [Laughter.]

Mrs. **Lloyd**. We would appreciate hearing from you, Mike. [More laughter.]

I only said you had to be brief. [More laughter.]

Mr. **McCormack**. Madam Chairman, I think Manny is putting me in a terrible position here.

Mr. **Lujan**. I don't mean to.

Mrs. **Lloyd**. We certainly welcome you. We miss you on this committee and we are still gaining the results of your valuable insight when you were a member of this committee.

Mr. **McCormack**. Thank you very much. It is always a joy to be back.

Manny, did you have a question you wanted to ask me?

Mr. **Lujan**. Well, you were the one that was proposing the FED to begin with. You were the author of the Magnetic Fusion Act. What would be your strategy now? We have a limited budget. There is no way of getting around the fact that it would be a very difficult thing to, as we ran into with originally, to be able to get something like that going. What would be your strategy at the moment?

Mr. **McCormack**. First of all, Madam Chairman, I want to say that I—please understand that I am speaking only for myself and only at the request Manny's invitation and yours and that I want to do nothing but answer the questions that you ask of me. You can't get me very far into this conversation without my having to state that I am now a member of the Board of INESCO, so anything I say is going to be prejudiced, or at least it is going to be construed as prejudice.

As you know, I have been very restive with the limitation on the budget. I have fought and fought and fought against the limitation on the budget. I resign myself to that, particularly for this coming fiscal year. For this reason, I think this committee has an exceptional responsibility. You know, I constantly look at this committee as the one committee that has been more innovative, more courageous than almost any committee on the Hill. That was our position in the past. Over and over again, trying to draw from the wealth of wisdom that was available to us, in this case, from the fusion community, we had to make the decisions. We tried our best to make the decisions when we wrote the law in 1980, and clearly we would probably write it differently today even if the budget were not restraining us. Nevertheless, this committee took the initiative and tried to create policies for the country.

I think this is the important thing that you, Madam Chairman, and the rest of the members of the committee must remember today, that it is this committee's responsibility to make these decisions based on all the information that you can get, but also based on the political reality you are facing.

I am trying to get around answering your question, Manny, but the budget is not going to change very much, I don't think, in the coming years. Quite frankly, I don't see how in the world you can plan to build a $1 or $2 billion machine in the present atmosphere and with the present programs. I don't know what the thoughts are in the minds of all the individuals who are contemplating this. Obviously, it would be a nice machine to have, but whether or not we can get it in the present atmosphere without something giving pretty seriously someplace else, I don't know. I think that this has got to be a matter of very serious consideration.

Mr. LUJAN. Do you have any thoughts about the small machines?

Mr. MCCORMACK. Yes, I do. Of course, I do. I really believe that the small machine should be explored very seriously at this time, perhaps several small machines. I think that Dr. Furth is being not completely fair in his statement when he equates that work that had been done by INESCO with a number of other ideas that have been put forth. I sat, as chairman of this committee, and heard many ideas brought to me which we recognized were totally unrealistic, but INESCO does not fall in that category.

I chair a panel for INESCO from those brilliant physicists and engineers, electrical engineers, and structural engineers and heat transfer engineers in this country. We meet periodically with the scientists from INESCO to review their work. I am really the only scientifically unqualified person on that board, and the response from that committee is not one of charging out of the room and saying, great, we have solved all our problems. It is one of saying, essentially, this work is good, it is responsible, and we do not find any criticism of it. I think that puts INESCO's work in a unique position.

You are forcing me to intrude upon Dr. Shanny's testimony. I am trying to be as gracious as I can to answer your question without——

Mr. Lujan. Regarding the question of small machines, I am just concerned——

Mr. McCormack. I could take that completely out of context and talk about OHTE or another machine, where Dr. Ohkawa has done brilliant work and where there is certainly a wonderful chance for success there or perhaps several other models. It seems to me that Congresswoman Lloyd and Congressman Fuqua, in their leadership in moving toward small fission machines, have provided a very valuable service to this country. I think it is critically important. Next generation machines for fission I think will be small machines. They will be the goof-proof, prepackaged, prelicensed fission machines.

I think we need to look at that same sort of thing for fusion. We need to look at anything we do from now on—we have to take into consideration what is the commercial goal. Where are we going? Why are we all here? It has to be, when can we produce commercial power the most realistic way that it really is going to be useable out in the world?

I hope I have answered your question.

Mr. Lujan. Thank you, Mike.

Mr. McCormack. Thank you, Madam Chairman.

Mrs. Lloyd. Thank you very much, Mike. We will continue to reap the benefit of your wisdom, and we thank you very much for being here today.

Our next panel is Dr. Al Mense from McDonnell Douglas Astronautics, representing the Institute of Electrical and Electronics Engineers; Mr. Len Reichle of EBASCO; and Dr. Robert Gross, dean of SEAS of Columbia University.

We welcome you gentlemen to our panel today. I believe we do have all of your complete testimony, so you may summarize or proceed as you wish.

Al, it is good to see you back.

Dr. Mense. It is a pleasure to be back.

STATEMENT OF DR. ALLEN T. MENSE, INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS

Dr. Mense. Madam Chairman, Mr. Lujan, let me in the interests of time today try to move quickly through my testimony, and then just briefly touch on just a few topics that maybe are not normally brought up during the course of discussions on fusion.

I am appearing this afternoon on behalf of the Energy Committee of the Institute of Electrical and Electronic Engineers. The IEEE is the world's largest engineering society, with over 250,000 members, 200,000 of whom live and work and pay taxes in the United States. Eighty-two percent of our membership is employed in industry, so if I begin to talk with a bit of an industrial bias, it is because our membership is industrially biased. We do have 8 percent of our members in academic institutions, 10 percent in Government laboratories. We are certainly pleased to have the opportunity to appear here today.

Rather than try to cover the entire gamut of fusion-related activities, we have chosen to focus our attention on two topics. First, we would like to discuss a few reasons why we believe that a broad-base fusion energy program should be pursued, even if one assumes, as some people do today, that the energy resources that the United States has are adequate for the foreseeable future. I think today, more than ever, we are a little sensitive to whether that statement is true with the Mideast situation, and we can all look at the indexes of the oil company stocks taking little jumps every day.

Second, we would like to discuss the advisability of the fusion program moving on with the Tokamak fusion core experiment.

Let me get on with the first point. First, I think it is important for us to understand that fusion energy development cannot and should not, at this time, be linked to energy growth projections, and the reason is pretty clear. Old powerplants have to be replaced by new ones. What we would like to contend is that fusion has the potential to produce the best environmentally sound powerplant available.

With that in mind, let us talk about a couple of other points. I don't particularly want to go into the environmental issue. It is one that everybody has talked about before. Let's talk about some others.

First, we believe that fusion can enhance the Nation's security. The ultimate commercialization of fusion will provide the United States with an energy source which can, in the short run, allow us to export more primitive fuels, such as coal, thus an avenue to improve our balance of payments. In the longer run, fusion reactors will be sold. Both of these effects, of course, will relieve one source of world tension, the uneven geographic distribution of primary fuels. The sooner we get on with the technology to do that, I think, the better off we are going to be in the long run.

Fusion development also should be pursued because, in our belief, it contributes to a sense of national pride. That is something that scientists don't necessarily tune in with, but you as Congressmen have to deal with the citizens all the time, and their belief and understanding and feeling about their country and where their country is going and what their country is doing. We believe that national pride is, in fact, the citizen's feeling about his country, and that positive feelings contribute to productivity, social rest, economic progress, and general well being. The United States has traditionally been a country whose national pride and self-esteem have revolved around improvements by technological innovation. Examples, of course, include the Apollo program and the Space Shuttle.

The more challenging and useful the technological advance, the greater the benefit in improved national pride. Fusion, I think we will all agree, is viewed internationally and in the news media as the most challenging of mankind's technical problems. Energy development is obviously useful. A country that accepts this challenge will show recognizable world technology scientific leadership in a cause that should lead toward world peace and harmony and, I think, stand the United States in good stead.

The other thing we have is that fusion can contribute to a better economic picture in the long run. As fusion is potentially a multifaceted source of energy production, electricity production, high temperature heat processing, synthetic fuel hydrogen production, fissile fuel production, the list can go on. As such, it has the potential to provide competition for existing methods and thus add diversity to the marketplace. Diversity leads to market stability, leads to economic strength, secures and assures national defense, and provides jobs.

I think those of us who were in Tennessee when the coal miners went on strike we were glad we had some fission plants on line. I would like to think when the uranium miners go on strike in the year 2050, that we have got some fusion powerplants on line so that we don't get ourselves caught. I think that is a commonsense approach and goes to illustrate the importance of having diverse sources for a lot of things, but energy in this case.

Obviously, this research program has attracted the best and the brightest of minds. It has attracted them because it is a program that looks like it was going somewhere. One of the problems that we are having today to wrestle with is, is, in fact, the program moving forward? If your program doesn't move forward, it is very tough to attract the best and the brightest of minds who are, in fact, the people we need to have to make sure the technology is a success.

The second issue we want to deal with is to discuss the viability of going forward with the Tokamak fusion core experiment. Let me not mince words. The IEEE Energy Committee fully endorses beginning work on the Tokamak fusion core experiment. We believe the only experimental data base that is sufficiently developed to allow a major advancement to the engineering stage is that of the Tokamak concept. Thus, we believe it is a very logical step to take.

Fusion has made extremely good technical progress over the past few years. However, from a programmatic point of view, it is still sitting at the same crossroads where it was in 1980. I think what we have to admit is that fusion has not been able to establish itself as a high enough national priority in the eyes of this administration to warrant their requesting increased funding.

I think we would all agree that in tight budget times, holding the line on spending is very reasonable, so the logical question is, how have other programs fared in this tight budget climate? Certainly, a lot of the other energy programs have not fared well and have not done anywhere near as well as fusion has in keeping the numbers it has. On the other hand, if we compare the fusion program budget to the other programs inside the Office of Energy Research, which, if you will, are its sister programs on the basis of science, then fusion has fared the least well.

In real dollars, the other programs in the Office of Energy Research have budgets that are now $0.5 billion per year more than they were in 1982. Clearly, I think the evidence is inescapable. While fusion is still alive and spending a great deal of money, it must be considered important enough to be moved ahead, at least by this administration.

I think the key question becomes one of, is that the continuing wish of Congress? To add money to a program that has no clearly defined end goals is certainly not appropriate. Here is where the program meets a dilemma: what should the next goal be?

The answer, fortunately or unfortunately, depends a lot on what you expect the budget level to be. If you wish to put a lot of money in, you can do a lot of things. If you don't wish to put a lot of money in, you cannot. However, as responsible Members of Congress, you certainly are not going to write a blank check until you know what the goals are. Unfortunately, the administration, in its desire to hold at least the fusion budget down, has prevented the fusion program from voicing its desired goals. Meanwhile, the program, which is having technical success, is chomping at the bit. The result is frustration.

If this frustration is not abated, the program stands a good chance of decaying, and I would have to say a little sarcastically, the United States will have demonstrated again its rare ability to achieve failure when victory is in hand. I would hope that this committee, as has been in the past, would not let that happen.

Fortunately, within the last month, two review committees, which report to the Department of Energy, have recommended that fusion must have a new lead project. The details of these committee reports are covered by other witnesses and will not be reiterated here. This lead project, if selected at this time, can only be a Tokamak, and it is with some courage that the fusion community has endorsed this step.

The courage comes from the realization that fusion faces two very real risks. The first is that, if the program does not proceed with a path which clearly leads to an energy production goal, that the Congress, or maybe the administration itself, will cannibalize the program for funds to use elsewhere. The justification of such an action would be the program's lack of a direct energy relevant focus.

The second risk is a more long term one, but one that Congressman Lujan brought up earlier today, and that is the risk that the program will move forward prematurely with one particular approach, only to find out in future years, that that approach lacks potential to be economically competitive. Meanwhile, if one reads between the lines, other backup or alternative concepts would, of course, have been killed off for lack of funding.

Both risks are real. The fusion program, under a tightly constrained budget situation, has been forced to choose. This is not necessarily the path they want to go down if they had the money to tell you what their programmatic goals—the ones they would like to see. However, in a tight budget climate, you force the folks to choose, and they have chosen. They appear to believe the first risk, the one that fears that their program will be cut if they don't have a clearly discernable goal, is the most threatening, so they have endorsed moving ahead.

The IEEE, in fact, approves of this and we endorse that position. If we were to not move ahead with the next major Tokamak experiment, it would be a little like telling the Wright brothers that, based upon aerodynamic scaling laws as were known at that time, they should not perform their proof of principle experiment, in other words, that they should not fly their heavier than air aircraft, because the plane does not extrapolate to a safe, viable, economic, 100 passenger, commercial airliner. Fortunately, the Wright brothers moved ahead, proved the principle, and future airliners evolved from their start and that beginning. It seems unreasonable to believe that fusion, unlike every other technology worked on to date, must give birth now to a full blown commercially capable device.

That, we believe, is an unreasonable request. We cannot guarantee that the decisions you make or that we help you make or are made by the administration will lead us down the easiest path to fusion. There are no guarantees. All we can say is that technology developments and the spinoffs and benefits that come from those seem to progress most successfully when strong and determined decisions to move ahead have been made. We believe an endorsement by Congress to make the Tokamak fusion core experiment a project line item in 1985 or in 1986 is imperative. If we don't, we believe that the continued success of fusion is in real doubt.

I would answer any questions you may have.

[The prepared statement of Dr. Mense follows:]

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STATEMENT ON FUSION ENERGY RESEARCH

BY

DR. ALLAN T. MENSE

on behalf of the
ENERGY COMMITTEE

of the
INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS, INC.

Delivered before the
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

HOUSE SCIENCE AND TECHNOLOGY COMMITTEE

FEBRUARY 29, 1984

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DR. ALLAN T. MENSE
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FEBRUARY 29, 1984

I am Allan T. Mense, Senior Scientist with the Fusion Science Department of the McDonnell Douglas Astronautics Company. I am appearing before you this afternoon to present testimony on behalf of the Energy Committee of the Institute of Electrical and Electronics Engineers (IEEE). The IEEE is the world's largest engineering society, with nearly 250,000 members worldwide, 200,000 of whom live and work in the United States. Approximately 82% of our membership is employed in industry, 8% in academic institutions, and 10% in the various government laboratories and agencies. The Energy Committee itself has representatives from 32 different technical societies (within the Institute) and 6 different geographical regions. The Committee is vested with the authority to develop the Institute's energy policy and, once that policy is adopted by the Institute, we are one of the major mechanisms by which the policy is voiced in public.

We are pleased to have been given the opportunity to appear before you today and address the very important area of fusion energy development. We continue to believe that fusion possesses the highest potential of all presently known energy sources and wish to commend this Subcommittee for its strong support of the fusion effort over the years.

Rather than cover the entire gamut of fusion related activities, we have chosen to focus our attention on two key, and we hope timely, issues. First, we wish to give a few reasons why we believe broad base fusion energy research should be pursued even if one assumes U.S. energy resources are adequate for the foreseeable future.

Secondly, we wish to discuss the advisability of the fusion program moving on towards a major new device - the tokamak concept called the Tokamak Fusion Core Experiment - at this time.

ISSUE #1

WHY DEVELOP FUSION NOW?

- Fusion is Potentially the Best Base Station Power Plant

  To fully realize fusion's ultimate potential as an Energy source, it is important to understand that fusion energy development cannot and should not be linked to energy growth projections or forecasts. The reason is rather straightforward. Old power plants will always have to be retired and replaced with new ones. Fusion should be developed because it has the potential to be the best centralized base station power plant, not just another way to provide power. In other words, fusion has the potential of providing the highest quality energy, and quality has been important in many past developments. There is no reason to expect it not to be so with energy. Thus, we would caution against waiting until the next world energy crisis before deciding (possibly too late) that fusion development is needed. The second caution would be that fusion is not developed until one actually builds the facilities; concepts still on paper are not technologies well in hand.

  With the preceding comments in mind, let me proceed to discuss the other fundamental reasons for developing fusion, even if we believed the country had sufficient other resources to provide us all with a so-called "carefree" energy future.

- Fusion Has the Potential to Provide the Most Environmentally Acceptable Energy Source

  The currently used fuels (primarily fossil) that provide the potential for a "carefree" energy outlook also present serious environmental concerns. The concerns range from the scarcity of water for coal slurry pipelines and/or the need for more railways, to the production of acid rain, exacerbation of breathing related disorders, increasing black lung disease, and enhancing the "greenhouse" effect.

Every other energy source, including uranium, geothermal, hydropower and even solar, has its own peculiar environmental liabilities. Although it is premature to be sure, it is the concensus of the technical community that fusion energy will have substantially less environmental liabilities than competing energy technologies.

o Fusion Research Can Enhance the Nation's Security

The ultimate commercialization of fusion will provide the U.S. with an energy source that can in the short run allow us to export more primitive fuels (e.g. coal) much as we do now with excess food. This is an avenue to improve our balance of payments. In the longer run, fusion reactors will be sold worldwide. Both of these effects will relieve one source of world tension, the uneven geographic distribution of primary fuels. In fact, it has been argued that it is immoral for the U.S. not to devlop and use its high technology energy sources itself and, thus, help to relax the market for fossil fuels so that third world countries (who possess large amounts of strategic materials the U.S. needs) can use these fuels to increase their standard of living. Petroleum and synthetic (fossil) fuels should be saved for use as portable (transportation) fuels for which there are no easy substitutes.

- Fusion Development Contributes to a Sense of National Pride and Well Being

  National pride is a citizen's feelings about his country. Positive feelings contribute to productivity, societal rest, economic progress and the general well being of the nation. The U.S has traditionally been a country whose national pride and self esteem have revolved around improvements by technological innovation. Some examples are the Apollo program and the Space Shuttle. The more challenging and useful the technological advance, the greater the benefit in improved national pride. Fusion is viewed internationally and in the news media as the most challenging of mankind's technical problems. Energy development is obviously useful. A country that accepts this challenge to develop fusion will show recognizable world technology/scientific leadership towards a peaceful goal.

- Enhance Our Economic Picture

  As fusion is potentially a multifaceted source of energy (e.g. electricity production, high temperature heat processing, synthetic fuel/hydrogen production, fissile fuel production), it will provide competition for existing methods and, thus, add diversity to the market place. Diversity leads to market stability, produces economic strength, secures and assures national defense, and provides jobs. The multiplicity of end uses for fusion also reduces the risk of developing a technology that will not have a market. Widespread market penetration of new major energy sources has traditionally taken from 50 to 100 years (e.g., wood giving way to coal); therefore, if fusion is to be commercially available in 50 years, now is the time we must start. Another attraction of fusion is that its development will bring new technology companies into the power plant production market place. This will be good for the utilities and the consumer. Pursuing fusion technology development will create new spin-offs in technology development similar to those of the space program.

- Fusion Development Advanced Leading Edge Technology

  Fusion research has attracted some of the best and brightest minds in the country and has drawn the best of students into the science/engineering disciplines. If the program now loses momentum, this drawing of new bright talent will slow. Also, fusion can give back a good name to nuclear energy and help keep science/engineering in the educational forefront.

  We hope our brief synopses given in the preceding paragraphs will help you, the members of this Subcommittee, in the continuation of your efforts to move fusion research into a higher national priority. To aid in this, we have included a brief outline of our reasoning as an appendix to this testimony.

  For these reasons, we believe that the fusion research community, which enjoys the happy situation of having many excellent ideas relevant to the eventual choice of a superior power source, should be supported at a sufficient level to assure the timely investigation of these relevant matters.

Let me now move on to the second major issue we wish to discuss.

ISSUE #2

SHOULD THE U.S. FUSION PROGRAM MOVE FORWARD NOW WITH THE DESIGN AND CONSTRUCTION OF AN IGNITION DEMONSTRATION EXPERIMENT?

The IEEE fully endorses beginning work now on the Tokamak Fusion Core Experiment (TFCE). The only experimental data base that is sufficiently developed to allow a major advancement to the engineering stage is that of the tokamak concept. Thus, an advanced Tokamak Reactor is the logical candidate at this time. Funding its development is appropriate at this stage.

While this is an easy position to take, and maybe even a popular one, the Energy Committee did not arrive at it easily. Let us share with you this afternoon some of our reasoning.

Fusion has made good technical progress over the past few years. However, from a programmatic point of view, it is still sitting at the same crossroads where it was back in 1980. It would be easy to say that lack of money was the only culprit in bringing about this stagnation. Actually, it is more appropriate to say that fusion was not able to establish itself as a high enough National priority in the eyes of the Administration to warrant their requesting an increase in funding. In tight budget times, holding the line on spending seems reasonable. The logical question is then, "How have other programs faired?" Some energy programs have done much worse than fusion. On the other hand, a comparison of fusion with its fellow programs within the Department of Energy's Office of Energy Research clearly shows that, since 1982, fusion has faired the least well. For the record, the actual increases are shown below in Figure 1.

	1982	1985	3 Year Change
High Energy Physics	364.5	561	+ 54%
Nuclear Physics	133.2	184	+ 38%
Basic Energy Sciences	241.2	420	+ 74%
Magnetic Fusion	451.2	483	+ 7%

FIGURE 1. Comparison of Office of Energy Research Budgets

The conclusion is inescapable, while fusion is still alive, and spending a great deal of money, it is clearly not considered important enough to be moved ahead. (We might note that Congress has added small but significant amounts of money to Magnetic Fusion each year since 1982.) Since DOE clearly has added money to some research programs, this would indicate that all the other OER programs have a higher priority than fusion. The key question becomes, "Is that the continuing wish of Congress?"

To add money to a program which has no clearly defined goals is certainly not appropriate. But it is here where the program meets a dilemma - what should be the next major near term goal? Fortunately or unfortunately, the answer depends on the expected budget levels over the next decade. But you, as responsible members of Congress, aren't going to write a blank check until you know clearly what the goals are. Unfortunately, the Administration, in its desire to hold the budget down, has prevented the fusion program from voicing its desired goals. Meanwhile the program is technically chomping at the bit. The result - frustration. If the frustration is not abated, then the program stands a good chance of decaying, and the U.S. will have demonstrated again its rare ability to achieve failure when victory is in hand.

Fortunately, within the last month, two review committees, which report to the Department of Energy, have recommended that fusion must have a new lead project. The details of these committee reports (the Magnetic Fusion Advisory Committee and the Energy Research Advisory Board) are covered by other witnesses at today's hearings and will not be reiterated here. This lead project, if selected at this time, can only be a tokamak, and it is with some courage that the fusion community has endorsed this step.

The courage comes from the realization that fusion faces two very real risks. The first risk is that, if the program does not proceed with a path which clearly leads to an energy production goal, Congress (or maybe the Administration itself) will cannibalize the program for funds to use elsewhere. The justification for such an action would be the program's lack of a direct energy relevant focus; i.e., no one will support large fusion budgets on the basis that it produces just plain good science. The questions for Congress are numerous. Would you support fusion on the basis that it is good science? Would you support it at the same or increased budget levels? For how long?

The second risk is a more long term risk, but one with some historical precedent. The risk is that the program will move forward prematurely with one particular approach only to find out in future years that that approach lacks the potential to be economically competitive. Meanwhile, the other backup or alternative fusion approaches will be killed off by lack of funding. Would Congress maintain funding for a balanced base program? What if the lead mainline project overran its cost? From where would the additional monies come?

Both risks are real. The fusion program, due to the tightly contrained budgets of recent years, has been forced to choose. The program appears to believe that the first risk (i.e., near term budgetary instability) is the most threatening and has endorsed moving ahead with a new tokamak - the Tokamak Fusion Core Experiment.

The IEEE endorses this decision, but with the qualification that reasonable funding levels must be maintained in the universities and in the most promising of alternate concepts to help mitigate the long term technical risk. The budget, as proposed, covers this long term risk, but fails to initiate engineering development recommended. The IEEE recommends increasing the budget to include TFCE.

Within our present understanding of fusion physics, large tokamaks do not appear to be capable of penetrating the electrical power production market place in the early 21st Century. However, if we're not to move forward with the next major tokamak experiment, it would be a little like telling the Wright brothers that, based upon aerodynamic scaling laws, as known at that time, they should not perform their proof-of-principle experiment (i.e., fly a heavier-than-air craft) because their plane does not extrapolate to a safe, viable, and economic 100-passenger commercial airliner. Fortunately, the Wright brothers moved ahead, proved the principle, and future airliners evolved from their start. It seems unreasonable to believe that fusion, unlike every other technology worked on to date, must give birth to a full blown commercially capable device.

We submit that fusion cannot and should not be asked to answer questions of economic potential at this time in the program's development. We must learn the physics of fusing plasmas. We must learn the engineering of fusion devices. This information will benefit all concerned, even the non-tokamak advocates.

We cannot guarantee that decisions made now will lead us down the easiest path to fusion. All we can say is that technology developments seem to progress most successfully when strong and determined decisions to move ahead have been made. We believe an endorsement by Congress to make the Tokamak Fusion Core Experiment a project line item in the FY 85 or FY 86 budget is an imperative for the continued success of fusion and the advancement of science for mankind.

Thank you again for giving the IEEE this opportunity to testify and I will be happy to try to answer any questions you may have on the fusion program and this testimony.

WHY DEVELOP FUSION

- IT HAS A MULTIPLICITY OF END USES

  i) Electricity production (safe, environmentally acceptable, nearly zero fuel cost.
  ii) High temperature heat processing for industrial and chemical manufacturing.
  iii) Hydrogen production (transportation fuel of 21st Century).
  iv) Breed fuel for LWR nuclear reactors.

- DIVERSITY IS EXTREMELY IMPORTANT

  i) Leads to market stability
  ii) Produces economic strength
  iii) Secures the national defense
  iv) Provides jobs

- THE U.S. HAS SPENT OVER $3 BILLION ON FUSION AND THE PAYOFF TO THE TAXPAYER REALLY ONLY COMES IN THE ENGINEERING DEVELOPMENT AND COMMERCIALIZATION PHASES

  i) Fusion Engineering R&D produces cross-disciplines which will strengthen industrial capabilities.
  ii) As in the space program, spin-offs occur and are implemented into the market place very rapidly (primarily with industrial involvement).

  iii) Commercialization depends on the strength of the market, National needs, many factors we are unable to predict now. No fusion - no choice.

- NEW INDUSTRIES NOT PREVIOUSLY IN THE ENERGY PRODUCTION BUSINESS WILL BE MAJOR PLAYERS IN THE FUSION PROGRAM

  i) These new industries will supplement the present industrial base.
  ii) These newer industries will bring more exacting engineering skills, which will produce a healthy competition in the power production business.
  iii) These new industries have long track records of concern for public safety.

- IF WE DON'T, OTHERS WILL

All world fusion programs have been experiencing almost level funding for the past 2 years. However:

i) Since 1977, the U.S. fusion budget has increased at a rate less than 5%/year to the present level of $471M.
ii) Since 1977, Japan's budget increased by over 30%/year to the present level of $300M.
iii) European budgets increased by nearly 100% over the previous 5-year budget cycle.
iv) Russians employ almost twice as many fusion scientists as the U.S., and they pursue all major concepts simultaneously.

Mrs. LLOYD. Thank you, Al. I see you have not lost your vivacious spirit or your enthusiasm. We hope that you don't. We all find it very refreshing.

Another one of our dear, dear friends is Len Reichle. We welcome you also.

STATEMENT OF LEONARD F. C. REICHLE, EXECUTIVE VICE PRESIDENT, EBASCO, INC.

Mr. REICHLE. Thank you. Madam Chairman and members of the subcommittee, I am Len Reichle, group vice president and member of the board of directors of Ebasco, the company that successfully engineered and built the Tokamak fusion test reactor, the TFTR, at Princeton. Also, I am chairman of your advisory panel on fusion energy.

You have asked my views on three topics:

No. 1. How can the DOE magnetic fusion funds best be spent to support both the scientific and engineering thrusts of the program?

In general, I believe the Government funds can best be spent if they are directed toward the achievement of the objective of ignition and long pulse equilibrium burn. This requires designing, building, and operating, not just studying, a large fusion device beyond the capability of TFTR. Only in this manner can the objective of a demonstration of ignition and long pulse equilibrium burn be accomplished.

In this connection, I support strongly the September 23, 1983, recommendation of the Magnetic Fusion Advisory Committee that a Tokamak fusion core experiment, the TFCX, be initiated and the February 3, 1984, recommendation of the Energy Research Advisroy Board, ERAB, that DOE commit to building what it calls a burning core experiment, the BCX basically the generic model of TFCX.

Accordingly, I am pleased that DOE has authorized the Princeton Plasma Physics Laboratory to issue an invitation to industry for TFCX conceptual design support, and I hope your committee will provide continuing and priority endorsement to this project.

However, while TFCX is being initiated, it is important at the same time to maintain a strong base program in magnetic fusion. Such a program is needed to explore new approaches, to expand our data base on current approaches, and to support the TFCX. I believe an adequate base program can be maintained even within the projected relatively flat budgets for magnetic fusion.

Your question No. 2, what activities should be initiated to support the TFCX, and what specific funding should be provided in fiscal year 1985?

I believe the activities to be initiated in support of the TFCX should be determined by the respective industrial teams selected by PPPL to define TFCX conceptual designs. Of course, such support assistance to the industrial team must be subject to budgetary and other limitations of PPPL and other laboratories and universities participating in the DOE fusion energy program. However, let's turn our industrial creativity loose in a competitive arena, with the full support of the laboratories and universities of our fusion community, and watch the progress that will be made when the industrial teams are made responsible for the designs that will emerge.

In 1976, Ebasco was selected out of a strong competitive field, and was made contractually responsible for the conceptual and detailed engineering design, procurement, installation, and assembly of the TFTR. The project was completed on schedule, and is operating beautifully. Let's use this pattern of success again.

As to what specific funds should be provided in fiscal year 1985, I would like to say only, don't be too restrictive in the funds made available to the competing industrial teams, or in the dedication of laboratory and university staff to provide scientific and engineering assistance to the industrial teams.

It currently is "guesstimated" that the TFCX is likely to cost about $1 billion of PACE funds alone. For such an investment, in times of severe budgetary restraint, I believe the selected design and its cost estimate will be most credible in the eyes of DOE, OMB, and the Congress if the design and cost estimate come from a strong industrial team which has been fully responsible for the design and the cost estimate. In general, the more money invested in conceptual designs, the better the project can be evaluated, and the more realistic its cost estimate becomes.

Your question 3: How can the vitality of the program not only be preserved, but enhanced?

I believe the vitality of the program can be preserved and enhanced by continuing to expand the meaningful participation of industry in the magnetic confinement fusion program. This must be done in a manner that does not threaten the roles of laboratories and universities. Let each entity of the fusion community do what it can do best. Our great goverment-funded universities and laboratories clearly excel in fusion physics, in new concept definition and experimentation, and in the fusion program development, and program management.

Industrial companies excel in systems integration, design engineering, engineering development, procurement, installation and assembly, QA, project management, and operations. Let us continue to work even more closely together in planning and execution of the Nation's fusion energy program.

Thank you.

[The prepared statement of Mr. Reichle follows:]

TESTIMONY

to the

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

of the

COMMITTEE ON SCIENCE AND TECHNOLOGY

of the

U. S. HOUSE OF REPRESENTATIVES

by

Leonard F C Reichle, Group V-P
Ebasco Services Incorporated,
an ENSERCH Corporation
February 29, 1984

Madame Chairwoman and Members of the Subcommittee:

I am Len Reichle, Group Vice-President and member of the Board of Directors of Ebasco, the company that successfully engineered and built the Tokamak Fusion Test Reactor (TFTR) at Princeton. Also, I am Chairman of your Advisory Panel on Fusion Energy.

You have asked my views on three topics:

1 - How can DOE Magnetic Fusion Funds best be spent to support both the scientific and engineering thrusts of the program?

In general, I believe DOE funds can best be spent if they are directed toward achievement of the objective of ignition and long-pulse equilibrium burn. This requires designing, building and operating (not merely studying) a large fusion device beyond the capability of the TFTR. Only in this manner can the objective of a demonstration of ignition and long-pulse equilibrium burn be accomplished.

In this connection, I support strongly the September 23, 1983 recommendation of the Magnetic Fusion Advisory Committee (MFAC) that a Tokamak Fusion Core Experiment (TFCX) be initiated, and the February 3, 1984 recommendation of the Energy Research Advisory Board (ERAB) that DOE commit to building what it calls a Burning Core Experiment (BCX) - basically the same kind of device as the TFCX.

Accordingly, I am pleased that DOE has authorized the Princeton Plasma Physics Laboratory (PPPL) to issue an invitation to industry for TFCX Conceptual Design Support, and I hope your Committee will provide continuing and priority endorsement to this project.

However, while TFCX is being initiated, it is important at the same time to maintain a strong base program in magnetic fusion. Such a program is needed to explore new approaches, to expand our data base on current approaches, and to support the TFCX. I believe an adequate base program can be maintained even within the projected relatively flat budgets for magnetic fusion.

2 - What activities should be initiated to support the TFCX, and what specific funding should be provided in FY 1985?

I believe the activities to be initiated in support of the TFCX should be determined by the respective industrial teams selected by PPPL to define TFCX conceptual designs. Of course, such support assistance to the industrial teams must be subject to budgetary and other limitations of PPPL and other laboratories and universities participating in the DOE Fusion Energy Program. Let's turn our industrial creativeness loose in a competitive arena, with the full support of the laboratories and universities of our fusion community, and watch the progress that will be made when the industrial teams are made responsible for the designs that will emerge.

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As to what specific funds should be provided in FY 1985, I would like only to say: Don't be too restrictive in the funds made available to the competing industrial teams, or in the dedication of laboratory and university staff to provide scientific and engineering assistance to the industrial teams. It currently is "guesstimated" that the TFCX is likely to cost about $1 Billion of PACE funds alone. For such an investment, in times of severe budgetary restraint, I believe the selected design and its cost estimate will be most credible in the eyes of DOE, OMB and the Congress if the design and cost estimate comes from a strong industrial team which has been fully responsible for the design and cost estimate. In general, the more money invested in conceptual designs, the better the project can be evaluated and the more realistic its cost estimate becomes.

3 - How can the vitality of the program not only be preserved, but enhanced?

I believe the vitality of the program can be preserved and enhanced by continuing to expand the meaningful participation of industry in the magnetic confinement fusion program. This must be done in a manner that does not threaten the roles of the laboratories and the universities. Let each entity of the fusion community do what it can do best. Our great DOE-funded universities and laboratories clearly excel in fusion physics, in new concept definition and experimentation, and in fusion program development and program management. Industrial companies excel in systems integration, design engineering, engineering development, procurement, installation and assembly, QA and operations. Let us continue to work even more closely together in planning and execution of the nation's fusion energy program.

Mrs. Lloyd. Thank you very much, Len. It is always a pleasure to have you with us. We appreciate your contribution over the years, as well.

Dr. Gross, I don't believe you have appeared before this subcommittee before, and we certainly welcome you today. We are appreciative of your being here and we look forward to your testimony now.

STATEMENT OF DR. ROBERT A. GROSS, DEAN OF FACULTY, COLUMBIA UNIVERSITY SCHOOL OF ENGINEERING AND APPLIED SCIENCE

Dr. Gross. Thank you Madam Chairman and members of the subcommittee. My name is Robert A. Gross and I am the dean of the faculty of the Columbia University School of Engineering and Applied Science. I am also a member of the Department of Energy Magnetic Fusion Advisory Committee, MFAC.

I want to take this opportunity to express my deep appreciation to you and others in the U.S. Government who have provided funds to support fusion research of my colleagues and me for nearly 30 years. Thank you for inviting me to give my views about the fiscal year 1985 DOE authorization request for the magnetic fusion program. The opinions that I express are mine alone, but I know they are shared by many others, particularly by those in research universities.

First, I strongly support the establishment now of the project to build the next major experimental machine, TFCX, whose objective is to achieve fusion ignition and sustained burn conditions. This machine is a major scientific experiment, and its success will mark a major milestone in a long series of plasma physics experiments which began in the 1950's. This experiment is really vital, for we need to have real experience with igniting and controlling burning plasmas. It will test our understanding of fusion plasma physics, and to be successful, it will also require excellent engineering.

Second, I must share with you the observation that there is significant concern within the fusion research community that, with relatively flat budget projections, establishing a new major project may seriously erode the so-called base program for fusion. It is the base program that funds nearly all university fusion research. It is the base program that supports many new ideas and the training of new fusion scientists and engineers. We all want to sustain U.S. leadership in fusion and TFCX is needed to achieve that objective. If, however, it is done at the expense of the base program funding, we will have done significant damage to the longer range future of this program. Increased funding for the fusion program can help alleviate that problem.

Mrs. Lloyd. We agree with you on that.

Dr. Gross. There is one specific recommendation that is very important and not very expensive. Fellowship support for new young plasma scientists and engineers who wish to enter this field is badly needed. Fusion is a high risk, long term program, and it is extremely important that we attract the best and the brightest people.

I urge you to provide new funding to the fusion program that will be specifically dedicated to the support of new graduate students who choose to devote their professional careers to fusion. I recommend that you provide funds for about 25 new fusion fellowships per year. Particular emphasis should be given to plasm physics experimentalists who are in extremely short supply. Finally, let me append to my written report a copy of the executive summary of the MFAC Subpanel Report that studied university roles in the fusion program. It contains a number of suggestions that were widely endorsed by the university fusion research scientists and by the MFAC committee.

I will be pleased to respond to any questions you may have.

Thank you very much.

[The prepared statement of Dr. Gross follows:]

Testimony of Dean Robert A. Gross, Columbia University
to
Committee on Science and Technology
Subcommitte on Energy Research and Production
Room 2318 Rayburn House Office Building

29 February 1984

Madame Chairwoman and Members of the Subcommittee:

My name is Robert A. Gross and I am the Dean of the Faculty of the Columbia University School of Engineering and Applied Science. I am also a member of the Department of Energy Magnetic Fusion Advisory Committee (MFAC). I want to take this opportunity to express my deep appreciation to you and others in the U. S. Government who have provided funds to support the fusion research of my colleagues and me for nearly thirty years. Thank you for inviting me to give my views about the Fiscal Year 1985 DOE authorization request for the Magnetic Fusion Program. The opinions that I express are mine alone, but I know they are shared by many others, particularly by those in research universities.

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There is one specific recommendation that is very important, and not very expensive. Fellowship support for new young plasma scientists and engineers who wish to enter this field is badly needed. Fusion is a high-risk, long-term program, and it is extremely important that we attract the best and brightest people. I urge you to provide new funding to the fusion program that will be specifically dedicated to the support of new graduate students who choose to devote their professional careers to fusion. I recommend that you provide funds for 25 new fusion fellowships per year. Particular emphasis should be given to plasma-physics experimentalists, who are in especially short supply.

Finally, let me append to my written report a copy of the Executive Summary of the MFAC Subpanel Report that studied university roles in the fusion program. It contains a number of suggestions that were widely endorsed by university fusion research scientists and by the Magnetic Fusion Advisory Committee.

I will be pleased to respond to any questions you may have. Thank you very much.

THE LONG-TERM ROLE OF UNIVERSITIES IN THE FUSION PROGRAM

A Report Prepared for
The Magnetic Fusion Advisory Committee
by
Subpanel 5

July 1983

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ACKNOWLEDGMENTS

Many knowledgeable, talented, and dedicated persons have contributed information and suggestions to this Panel. Thirty letters and memoranda were received from colleagues throughout the country. Oral presentations to the Panel were made by Dave Nelson, Rolf Sinclair, Leon Shohet, C. K. Chu, Dale Meade, Bob Borchers, Bill Drummond and Herb Woodson. The appendices on University Fusion Research in Japan and in Europe were written by C. K. Chu and Dieter Sigmar, respectively. The fusion program Ph.D. survey was conducted by Tom Stix. The Panel is particularly indebted to all these persons and with appreciation we acknowledge their contributions.

EXECUTIVE SUMMARY

Findings:

In response to the charge given to this panel we have found that university contributions to the fusion program have been significant, extensive, and cost effective. University fusion research has had a major beneficial impact on the national program. We give examples in the body of our report and detailed documentation in the appendices.

We find that universities have contributed to the national fusion program in several unique and important ways. First and foremost, they educate new fusion professionals; second, universities provide to the fusion program an extraordinary breadth and diversity of talent in the sciences and in engineering; and finally, university fusion research programs historically have been, and must continue to be, a major source of discovery and innovation.

We believe it is essential that university programs, focused upon fusion problems of high intellectual challenge, continue to be supported. We find that the present fraction of the national program that supports university fusion research is reasonable and provides a sensible university base program. We believe that fusion is extremely important to our nation's future, and the universities must be asked to give their very best to this program. Because of the great challenge and the great promise of fusion energy, the very best people, wherever they are found, must be engaged in our national program.

The role of universities in the United States fusion program must be considered in the context of the entire national effort. There are four complementary areas of activity: plasma research, fusion-technology, hardware development and construction, and the education and training of personnel. Performing these activities are the national laboratories, the universities, and industry. These three groups complement each other. The resources to attack the fusion problem are: personnel, existing laboratory facilities, and the technical capabilities of the nation. The challenge is to optimize the use of these resources. This optimization must take into account constraints such as: (a) funds limit the number of major paths that can be followed; and (b) machine time and accessibility limit the number of senior scientists that can usefully contribute to the operation of a single experiment. We believe that a number of critical issues can be successfully addressed on small- and medium-scale devices. It is within the context of this entire complex that universities, with professors, ambitious and bright graduate students, and post-doctoral associates, make their unique and prolific contributions.

In order to resolve issues critical to the development of fusion power, researchers in the national program in general and the university component in particular will require access to a spectrum of experimental devices, ranging in size from the relatively small devices typical of many existing university research programs to the very large facilities now found in the national labs. The former class of experiments have been and will continue to be useful in isolating and studying basic plasma phenomena, while the latter have the unique capability to create plasma with near-fusion parameters. It is important that small university programs, focused upon basic fusion problems of high intellectual challenge, continue to be supported.

As the plasmas produced in the large devices become more fusion relevant, the need to study basic phenomena with more advanced plasma parameters will also grow. Consequently, it will be necessary to build and operate a number of intermediate or "medium-scale" experiments whose primary functions will be to explore basic plasma behavior at substantially more advanced parameters than are possible with the present university-scale devices. In this context, medium scale refers to devices which will probably cost in the range of $3-30M. It is also clear that many of these devices should be sited at those universities where research competence and the capability to operate moderately large experiments exist.

The U. S. magnetic fusion project has, from nearly its beginning, contained research programs located at universities. Financial support for their operation is today at about fifty-one million dollars per year, which is about eleven percent of the total fusion operating budget. There are seven university programs which receive support ranging from one to twenty-two million dollars per year each, for a total of about forty-five million dollars per year. Twenty-nine universities have smaller programs whose total funding is about six million dollars per year.

University programs are the major source of new fusion scientists and engineers; between 70 to 80 new Ph.D.'s per year are trained. This approximately meets the present needs. In Appendix 5 we give the yearly production of fusion Ph.D.'s from universities that are supported by the fusion program, and we list the employers for these graduates. Although there is at present a reasonable balance between need and newly trained professionals, it is currently difficult to find new well-trained experimentalists. Competition from the Department of Defense, and some parts of the private sector, for persons trained by university fusion programs, is increasing.

Universities have made substantial contributions in both theory and experiments for the fusion program. For example, university scientists and engineers have contributed significantly in proposing, exploring, and developing basic theory, confinement scaling laws, impurity control techniques, sloshing ions for tandem mirrors, second stability regime physics for tokamaks, pumped limiters, special diagnostics, and current drive techniques. University fusion research has been innovative in proposing new magnetic confinement concepts. University-based programs have made major contributions to fusion technology by pioneering early reactor designs, suggesting new materials for use in fusion reactors, developing new blanket designs, and exploring the possibilities for advanced fusion fuels.

There are problems that some faculty in the university fusion community have perceived with DOE management of the program and there are serious concerns about the future. Some of the more widely held concerns that the university fusion community has transmitted to the panel involve:

a) the problems associated with budget changes and the desire for stability of university basic research programs. Fiscal stability is extremely important because of the need for continuity of graduate student support;

b) the character of the DOE review procedure. The criteria used in judging proposals should emphasize excellence, merit, and capability;

c) the need for better communication with the entire university fusion community and the dissemination of information;

d) the uncertainty of future manpower needs for the fusion program;

e) the management of small university programs. DOE should be flexible in its management approach to university programs so as to allow for the conception and development of innovative ideas;

f) the need to preserve the conditions which encourage free expression and criticism.

We have also examined how universities are used in the Japanese and European fusion programs. Universities have a relatively large role in the Japanese program and there is close collaboration with industry. In Europe, university involvement is modest, but where it exists there is close collaboration with Euratom laboratories. It is difficult to draw precise comparisons with the U. S. fusion program because of different funding methods, etc. Details about the Japanese and European university fusion programs are given in the appendix.

Recommendations:

The Panel recommends that a strong and vigorous university fusion program be continued. Specific recommendations are:

1. The unique capabilities of the universities in carrying out research and in training professionals for the fusion program should be utilized to the fullest. Adequate funding for this purpose must be a basic component of the fusion budget.

2. To continue to attract the best minds, the national fusion program must involve universities in fusion's most challenging research and development.

3. The magnetic fusion energy program should use universities in all aspects of the program. Some universities can undertake relatively large-scale research programs. Others can make effective contributions with smaller programs, including the single investigator.

4. There are problems in the fusion program which are best addressed with medium-sized experimental facilities. Consistent with recommendations 1, 2, and 3, some of these facilities should be sited at universities. Joint university-industry or national lab teams could be used to assist in the design, construction, and operation of these new experiments.

5. Plasma research in both theory and experiments needs to be sustained and encouraged. A core effort in theoretical and experimental non-programmatic basic research needs to be carried out because not enough is known about the physics of plasmas. Funds should be consciously dedicated to continue support of this basic plasma research. This support should be directed to problems of high intellectual challenge.

6. Research and development of fusion technology needs to be encouraged and expanded at universities where special engineering expertise exists, and some fusion engineering experiments located at those universities to attract bright new engineering students.

7. We find that the present fraction of the national program that supports university fusion research is reasonable and provides a sensible university base program. New medium-size experiments as recommended will require incremental funding above the base program.

8. There is need for more complete and timely communication between the DOE Office of Fusion Energy and all university fusion groups. A monthly newsletter should be published for the entire fusion program. It should include information about program changes, review panels, scientific and technical progress, personnel, etc.

9. Personnel exchanges between universities, national laboratories, and industry should be enhanced.

10. University fusion programs have a rather different character and purpose than those programs in national laboratories and industry. They explore new ideas and seek to understand the basic underlying science and technology. Flexibility is essential for university research and development programs. The Office of Fusion Energy is encouraged to manage these programs with that spirit and understanding.

11. Every effort must be made to provide stable and continuous funding for university programs. This is particularly important for smaller-sized programs, where longer range funding commitments are encouraged. Continuity of financial support is vital for graduate students.

12. The Magnetic Fusion Energy Technology Fellowships are valuable for attracting the best talent into fusion. Similar fellowships should be initiated for university programs supported by the Applied Plasma Physics and Confinement branches of the Office of Fusion Energy.

13. The universities are a valuable source of independent views and objective criticism. It is important to preserve conditions that encourage such free expression.

Mrs. LLOYD. Thank you very much, Dr. Gross. I certainly share your concern that we must commit more funds to the university community.

In my speech Monday I made reference to the fact that we must give greater support to the efforts of our universities and may rely on their evaluation and experience.

The MFAC report mentions universities' about DOE's review procedure, I believe, and that the criterion judging proposals should emphasize excellence and merit and capability. If we don't emphasize these criteria, what advice could you give this committee for improving this situation? We must not deemphasize the role of the university.

Dr. GROSS. Of course, one of the things that is extraordinarily hard in today's budget climate is for a new idea to be born and take root. I think that is the concern of some of my colleagues in some universities, that they have an idea, a concept that they feel very strongly about and of which very little is really known. They seek and wish to have the opportunity to try those ideas out. In the fiscal climate of 1984, that is very difficult to do.

Mrs. LLOYD. I certainly agree with your assessment. We hope that you will continue to give this committee your input and your thinking on our fusion program.

Len, as I remember, the organization that set out to build TFTR which we know is successfully completed was really a mix of personnel from PPPL and Ebasco, Grumman, Westinghouse, I think Allis-Chalmers, and others. It really took a lot of effort, as you well know, to get this mix to really function well. We don't want to have this problem as we move on to TFCX with this project.

Do you have any good suggestions as to how we might alleviate this problem? We don't want to make the same mistakes twice.

Mr. REICHLE. I think the way the Government and Princeton went about TFTR was basically very sound. What they did was to develop the concept and then they entered into a prime contract with Ebasco as the industrial subcontractor. Ebasco then had Grumman with us as our team subcontractor.

Then, in the course of defining the project in much greater detail, we entered into many other, more than 400, subcontracts, with Westinghouse, Allis-Chalmers, Rockwell, many, many other large companies and small companies. I think it worked out extremely well because responsibility was fixed on Ebasco as the prime industrial contractor reporting to Princeton. We had an excellent coordinating mechanism that Princeton set up so that our responsible project manager and design engineers interrelated with the corresponding people at Princeton, and it worked very well.

I think the one thing that we would recommend doing differently the next time is that the contract for design and construction of the facilies, like the buildings, instead of being handled separately and by DOE as it was in the case of TFTR, it should also be put under this main prime contractor. That would assure cost savings and greater continuing efficiency and economy in the total operation?

Mrs. LLOYD. So, you recommend one principal authority to delegate, to have the overall oversight responsibility and delegate.

Mr. **Reichle**. That is right. One major contractor reporting to Princeton who would be responsible for the validation of the conceptual design, going through detailed engineering design, procurement of materials and equipment, fabrication subcontracts, quality assurance, project management, startup and test, and this means engineering installation and assembly of everything including the buildings. Then you have one focus of responsibility reporting to Princeton.

Mrs. **Lloyd**. Thank you.

Al, you mentioned in your written testimony, I believe, that Tokamak was sufficiently developed that we could allow advancements to the engineering stage which of course means TFCX. Do you classify TFCX as an engineering device or would you state that it is a vital scientific experiment to achieve ignition?

Dr. **Mense**. First of all, let me put it this way. It is going to take a heck of a lot of engineering to build TFCX.

Mrs. **Lloyd**. Would you envision this device as having a blanket?

Dr. **Mense**. Well, in this particular device, the nuclear mission, which means the one of surrounding the device with a blanket and something like that is certainly not what is foreseen at this moment for TFCX as envisioned as a project to come under a $1 billion PACE limit. I think one of the challenges that the industrial players will have is to see what we can do to make this device flexible enough so that we can, in fact, expand on the device to do more ambitious things if either data should be different from the other experiments that would be providing data, or if budget limitations would be less restrictive and, therefore, we could go forward with a more ambitious situation.

I think that is going to take a great deal of creativity and I think the industrial teams are willing to do that. It is first and foremost a device to establish a physics mission that is important to have in order to set a reference point for real fusion reactors, be they small ones or large ones, be they mirrors, be they Tokamaks. It will set a data point on fusing plasmas.

But don't be mistaken into believing there is not a lot of really tough engineering that won't be done there, because there will. There is a lot of engineering that has to be done, so it will be an engineering device in that sense.

Mrs. **Lloyd**. Thank you very much.

Mr. Lujan.

Mr. **Lujan**. I don't have any questions, Madam Chairman.

Mrs. **Lloyd**. Thank you very much.

Thank you, gentlemen. I hope that you and all our panelists will agree to questions in writing because many of our committee members are not here today, and because of the number of panelists, I have had to restrict my questions as well.

Thank you.

Please welcome our next panel which includes Dr. Stephen Dean, Fusion Power Associates; Mr. Harold Motchan, McDonnell Douglas Astronautics Co.; and Dr. Ramy Shanny of INESCO. We welcome you today.

Please proceed, Dr. Dean.

STATEMENT OF DR. STEPHEN O. DEAN, PRESIDENT, FUSION POWER ASSOCIATES

Dr. DEAN. Thank you, Madam Chairman.

I think a lot of what needs to be said has been said. I note that the Magnetic Fusion Engineering Act of 1980 made a very clear set of goals and statements for the fusion program, which included as a planning strategy the initiation of a fusion engineering device to be operated by 1990. I supported that act at that time, and I continue to support that act as being the basis for planning the strategy of the programs even though funds are tight and we will have to slip our schedule somewhat from that in the act.

Shortly after the passage of that act, as you know, the Department of Energy, threw out that strategy, and after a long, painful process, delivered to you last fall a management plan that did not include a major device as the planning strategy in the near term for the program. I think what has happened is that the fusion community has, for several years now, been trying to fight that abandonment of that strategy, and now with the efforts of the Magnetic Fusion Advisory Committee and now the Energy Research Advisory Board has successfully or almost successfully been able to replace the comprehensive program management plan strategy with the original strategy which I support.

I believe that we must have a major facility as the rallying point for planning in the program.

I note that the Energy Research Advisory Board calls this facility a burning core experiment, and I think that that is an appropriate term. I would say that it is an equivalent, if not in total objectives at least in intent, equivalent of the fusion engineering device that was in the fusion act.

Having said that, I would like to say that it is also becoming clear that we do not have at the present time a design of a fusion reactor which is perceived to be commercially competitive. It behooves us not to ignore those who point this out to us. I believe we must seriously, and even more seriously now than in the past, attempt to seek concept improvements which will eliminate this impression that we do not have a commercially attractive product.

I believe it is essential if the fusion program is to maintain support, public support, that we be perceived to be going along a path that brings us into a competitive product position. Therefore, I think we must give very strong emphasis, in whatever program emerges in fusion to seeking an improved reactor concept so that we will remove this impression that we are not going along an appropriate path.

I think there are several ways of doing that. One is to attempt, within the Tokamak program, to continue to give priority to making a better and better object. That means higher performance per dollar. We have done several conceptual designs of next generation machines over the past 5 or 6 years. We change the name every year. Every year, it is encouraging that we get a little better at what it does per dollar.

I am not sure if we are there yet. I think we are probably not quite there yet. I am looking forward to the coming 6 months or couple of years in the design phase of TFCX to see if we can't continue to improve the performance of the machine relative to its cost. I hope that we can do that in a way that connects us with the future. I do not like to see this machine go forward simply as a roll forward machine which does better than we have done in the past. I think we must also connect it in to a path that looks like it is leading toward a better fusion reactor.

To that end, I believe that the Department of Energy should continuously update and fund their commercial reactor design studies. It is a source of concern to me that the last study we have done on a Tokamak reactor was the Starfire and that that was completed in 1980. I think it is a shame that we do not have an up-to-date Starfire reactor design to put on the table, and I believe it is very inexpensive to do these reactor design studies, and we ought to continue to build on them so that we can always have the latest thinking of how we are going to get to the end point. TFCX can then be judged in that context.

Therefore, I would urge that whatever design activities go on in the Department on TFCX and other types of systems that we urge the Department to continuously fund reactor design studies, like Starfire and like MARS. The MARS study that was completed on mirrors last year has been severely cut back. The Department considered it basically done and we would come back to it in a few years. They are doing a little bit of updating on it, but I believe that these kind of studies are worth their weight in gold. They do not cost very much and it is essential for our long-range program that we continuously update these studies.

The question of the alternate concepts, I think, that has come up and continues to come up is one that I would like to comment on. I believe that we have attempted to nurture these concepts at levels which the bulk of the community feels is appropriate. I feel that we do not give them as much advertisement and as much priority in our planning as perhaps they deserve.

Each of these concepts has immediate needs which should be taken care of. They are relatively inexpensive. We should continue to pursue them at levels which are appropriate for their stage of development. I think that the program attempts to do that, not always to the satisfaction of their advocates, at least they attempt to do it within the national laboratory framework.

I say that because having worked at the Department for many years, I have to admit that there is a bias in the Department against ideas which come from outside of the national laboratory framework. I think that it is appropriate for groups such as this committee to encourage the Department to take a broader view and to be a little bit more understanding of ideas that come from outside of its own network of people.

It is frequent that these ideas are not well developed and they cannot stand the intensity of a national laboratory peer review. Therefore, it is beholding to the Department of Energy, it seems to me, to be a little bit more understanding that these ideas sometimes cannot measure up to their standards of ideas that come from the national laboratories where they have a large number of people to analyze them in great depth.

So, I think we need to encourage entrepreneurship. I am encouraged by the activities of GA technologies and Philips on the OHTE.

I am encouraged by the ability of the stick-to-it-tiveness of INESCO to attempt to do something which is higher risk than the laboratories would do. I have to say that, in all honesty, it is very unlikely that you will ever get a proposal from a national laboratory or from the Department of Energy which is high risk. The planning assumptions and the priority assumptions of the national program is to be as low risk, and to be as maximally assured of success as possible.

I have to agree with Mr. Lujan that if things are reasonable in cost relative to the national budget, the program could well afford to take a flyer now and then in hopes of getting something that might pay off sooner.

I have been at a meeting of the Atomic Industrial Forum the last few days at which Mr. Keyworth spoke. It is quite clear that in spite of the apparent unanimity of our community here in urging you to do TFCX, that we have by no means yet closed the loop with the administration on the priority for this machine or for this program. Mr. Keyworth said that he views this program as a science program. It may be that this is a science program. It is certainly a science program, but it is much more than a science program. This program is very clearly motivated by the desire to produce a fusion reactor.

In that context, when you have private companies coming in with ideas that are going to be funded privately, you know that you are involved in a quest which is more than a science program. Therefore, I think we need to work with Mr. Keyworth and the Department of Energy to urge them to view this program as a research and development program with practical goals which we should be attempting to solve this problem and deliver a practical product in a short time, not in the timeframe of Mr. Keyworth which is the middle of the next century.

I was at the hearing last week when you asked Dr. Trivelpiece or Mr. Lundine asked Dr. Trivelpiece, what is the DOE target date for fusion? I tried to understand his response, but I think if you look at the transcript of that you will have a difficult time finding that he answered that question. In my opinion, the Department of Energy does not have a target date for fusion at this time. They want to embark, perhaps, on a next generation machine. It is not clear yet. But they certainly do not have any target date for the follow on activities or when this program will be brought to fruition. I believe that it is necessary for this committee to continue to urge the executive branch to get on with the job, not take 100 years at $500 million per year to solve this problem, and deliver a practical product.

I don't think it is necessary. I think the private ventures that you are starting to see is evidence that that is not necessary. I have heard a talk this morning from Dr. Grieger of Garching on the European strategy which is very much like the strategy of the Fusion Act. They have very definite goals and schedules in mind, and I think we need to reestablish those target dates in the planning of the Department.

Therefore, I think you should ask the Department to restructure their comprehensive program management plan to reflect the planning horizons of the ERAB panel and of the Fusion Act. If they can't meet the schedule of the Fusion Act at least they can deliver a practical product on some type of a reasonable schedule and not leave the program open ended, not way on into the next century.

Thank you.

[The prepared statement of Dr. Dean follows:]

FUSION POWER ASSOCIATES
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(301) 258-0545

STATEMENT OF
DR. STEPHEN O. DEAN
PRESIDENT
FUSION POWER ASSOCIATES
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY
FEBRUARY 29, 1984

It is a pleasure to have the opportunity to address your subcommittee again this year as you consider the Department of Energy's FY 1985 funding request for fusion energy research.

Fusion Power Associates is a non-profit corporation established in 1979 "to ensure the timely development and acceptance of fusion as a socially, environmentally and economically attractive source of energy." Forty-six industries, utilities, and universities and over 400 individuals are currently members or affiliates of Fusion Power Associates.

The Department of Energy request for magnetic fusion energy is primarily for continuations of existing strong efforts in tokamak and magnetic mirror confinement activities, combined with efforts in supporting research, technology and alternate concept development. The department's budget represents a reasonably balanced effort among the various disciplines and concepts required for fusion development.

We meet today at a time when the Alcator C device at MIT has recently achieved, for the first time in history, the quality of confinement required for net fusion energy release. This degree of plasma confinement, referred to as the Lawson Product, and expressed as the product of the plasma density and its energy confinement time, has been a major milestone goal of the fusion program for over 25 years. The achievement of this goal in late 1983 was an historic event.

Even as this event was occurring, however, early results from the next generation of fusion devices, TFTR in the U.S. and JET in Europe, were coming in. The energy confinement times already achieved in those devices, about three-tenths of a second, are about 10 times longer than in Alcator C, exceed our original expectations for those devices and give us further confidence that we are in position to proceed with the initiation of the next generation of device--to achieve ignition and the sustained release of fusion power. Such a device, if authorized now, could be operational in the early 1990's. Recently the DOE's Energy Research Advisory Board (ERAB) called the initiation of such a device (which they call BCX for Burning Core Experiment) "essential to sustain momentum and focus of the program." I support this recommendation.

"TO TAKE FROM THE SEA AND TO IMITATE THE SUN"

In calling for the initiation of BCX "even under the constraints of a constant fusion program budget," the ERAB added its voice to those of us who have been urging DOE to abandon the lackadaisical strategy it put forth in its so-called Comprehensive Program Management Plan (CPMP). Of the CPMP, the ERAB report states "the strategy implicit in the DOE Comprehensive Program Management Plan should not be followed" because "it imposes an unnecessary hiatus on the advancement of magnetic fusion and imposes an artificial schedule on the development of both mirror and tokamak programs, a schedule that could prove to be detrimental to both efforts." The ERAB report says the CPMP "is further unrealistic in that it calls for an ETR (Engineering Test Reactor), presumably with substantial engineering testing objectives, without providing for the prerequisite technology and engineering component development." As for the CPMP's statement that the 1980's will be a time of "product definition," with "product development" occurring in the 1990's, ERAB says that "This rationale is inconsistent with industry meaning of these terms and is misleading with respect to the experimental nature of fusion research over the next two decades." Fusion Power Associates has made these and similar criticisms of the CPMP beginning almost two years ago in our June 1982 newsletter and continuing to the present, as evidenced by our testimony October 20, 1983, at your hearing on the subject. I now urge you to formally reject the DOE's Comprehensive Program Management Plan and to require the preparation of a plan based upon the ERAB report and in conformance with the principles of the Magnetic Fusion Energy Engineering Act of 1980.

The Board of Directors of Fusion Power Associates has recently reviewed the present status of fusion development and urges that fusion energy development be based upon the following recommended policies:

- The expeditious development of practical fusion power should receive high national and international priority.

- The engineering aspects of fusion development require substantial additional effort now.

- Commitments to new and improved experimental facilities are urgently needed to insure continued program momentum and progress into the 1990's.

The Department of Energy should clearly state that its near-term objectives for the fusion program are to carry out integrated scientific and engineering activities to provide the basis for identifying and evaluating the most promising concepts for commercial application. There should be a heavy reliance on conceptual reactor design studies to guide the R&D program, which should be based upon clear program and project objectives, plans and milestones.

Although the requirements for commercially attractive fusion systems cannot be specified precisely at this early date, the need for improvements in present concepts is evident. It should be a program objective to aggressively seek concept improvements and to give this search high priority and high visibility in program discussions and decision making activities. It is not an indictment of the program that our concepts do not at present look commercially competitive; rather this fact should stimulate our search for promising evolutionary paths for our research.

Although physics programs require the bulk of fusion program funding at this time, a clearly-established and well-maintained reactor engineering program is equally essential. It should be an objective of the fusion program to ensure balanced effort and good coordination among the various essential elements of the program, e.g., physics innovation, parameter demonstrations, technology development, systems analysis and reactor conceptual design, and to stimulate the development of fusion capability in U.S. industry.

Finally, it should be a program objective to continuously improve and implement a coordinated management system. A strong and effective management system can accelerate pace by focusing efforts and reallocating resources.

FUSION POWER ASSOCIATES
POLICY STATEMENT
ON
FUSION ENERGY DEVELOPMENT

The Board of Directors of Fusion Power Associates, on behalf of the association, urges that fusion energy development be based upon the following

RECOMMENDED POLICIES

- The expeditious development of practical fusion power should receive high national and international priority.
- The engineering aspects of fusion development require substantial additional effort now.
- Commitments to new and improved experimental facilities are urgently needed to insure continued program momentum and progress into the 1990's.

The above recommended policies are based upon the following

FINDINGS AND OPINIONS

- We reaffirm our conviction that the principles and policies established by law in the Magnetic Fusion Energy Engineering Act of 1980 provide a sound basis for the expeditious development of practical fusion power.
- Scientific progress in fusion research gives increasing evidence that this new energy source will emerge early in the twenty-first century. There very likely will be a variety of paths to practical fusion power.
- It would be premature, at this stage, to judge which of the variety of magnetic and inertial fusion concepts will ultimately succeed commercially. This fact should not discourage use of the best available concepts in the design and construction of near-term fusion test facilities.
- The fusion programs in the U.S., U.S.S.R., Europe and Japan have comparable accomplishments, facilities and momentum. The present rate of progress in the world program is based on capital investment commitments made in the 1970's. Progress will predictably diminish unless new facilities are commissioned soon.
- International cooperative agreements have been a substantial factor in fusion progress and should be encouraged. Such agreements, however, are not an effective substitute for focused national efforts, national experimental facilities and test reactors.
- Governments should encourage and promote the full participation of industry in the planning, research and development as well as in the engineering and operating aspects of the fusion programs. They should also strengthen and encourage fusion-related programs in universities to ensure an adequate supply of engineers and physicists with knowledge in this field.
- The pace of fusion energy development today is clearly limited by the funds available rather than by science and technology. The skills required for engineering development exist today in a variety of institutions — industry, national laboratories, and universities. If these skills are properly mobilized and utilized, the time will soon come when society may reap the considerable benefits which fusion has to offer. These benefits include a virtually inexhaustible fuel supply, readily available to all nations.
- The availability and affordability of energy are essential elements to healthy and vigorous industrial society and are necessities for developing nations. Assured energy supply is an important determinant of world peace and security and a requirement for progress of future generations.
- We commend the governments of the world for making a sustained investment in fusion research. We believe the time has come to begin to capitalize on this investment by placing increased emphasis during the 1980's on the engineering and systems design aspects of practical fusion energy systems.

Fusion Power Associates is a non-profit, tax-exempt corporation established to ensure the timely development and acceptance of fusion as a socially, environmentally and economically attractive source of energy. To fulfill this purpose, we have adopted three primary goals:

- To ensure a smooth, timely transition from scientific research on fusion to engineering development and practical application.
- To foster cooperation in fusion research and development among all public and private organizations including government, universities, national laboratories and industry.
- To establish increased public awareness and understanding of the potential of fusion energy.

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Mrs. Lloyd. Thank you very much, Dr. Dean, for your statement. I share your concern and we certainly do hope that we can work in a positive manner with Dr. Keyworth as well. As I stated, if you were at the meeting when I spoke Monday, that we certainly do not intend to abdicate our responsibility, that we intend to live up to the mission of this committee and go forward with our fusion program.

Mr. Motchan, please proceed.

STATEMENT OF HAROLD MOTCHAN, VICE PRESIDENT OF ENGINEERING, McDONNELL DOUGLAS ASTRONAUTICS CO.

Mr. Motchan. Madam Chairman and members of the subcommittee, I welcome the opportunity to present ideas from an industrial point of view about the magnetic fusion energy program. I am accompanied today by Mr. Dale DeFreece and Bill Briggs who have worked in fusion energy for more than the 10 years that our company, McDonnell Douglas, has been a fusion contractor. It is requested that my prepared testimony be accepted for the record while I just very briefly summarize it for you.

Let me direct your attention to the chart that is enclosed in my testimony. It shows that we have been designing the next leading Tokamak experiment for 10 years. Every 2 years, we have discarded the design for various reasons and started over.

The startup of TFTR and JET and recent significant accomplishments shown in the upper right-hand corner of the chart are giving us the data base to proceed with the next Tokamak project. The TFCX design which Princeton is leading is well conceived. It is being done with an awareness of capital limitations, yet it will stretch the science and engineering communities to reach ambitious goals. It is essential that we stop the 2-year cycle and proceed dynamically to accomplish the TFCX project.

This, in brief, is my message. If you have further questions, I will be happy to elaborate.

[The prepared statement of Mr. Motchan follows:]

STATEMENT OF
MR. HAROLD MOTCHAN
VICE PRESIDENT ENGINEERING
MCDONNELL DOUGLAS ASTRONAUTICS COMPANY
TO THE ENERGY RESEARCH & PRODUCTION SUBCOMMITTEE
OF THE HOUSE SCIENCE & TECHNOLOGY COMMITTEE
ON 29 FEBRUARY 1984

The Tokamak Fusion Test Reactor (TFTR) was being designed at the Princeton Plasma Physics Laboratory in 1974 when McDonnell Douglas Corporation received its first fusion energy contract (Chart No. 1). In 1975, the project that would succeed the Tokamak Fusion Test Reactor was being studied by groups at Argonne National Laboratory, Oak Ridge National Laboratory, University of Wisconsin, and General Atomic (now GA Technologies). It was called the Experimental Power Reactor. We participated in that design study until, by the end of 1976, it was determined to be too ambitious. A generic "The Next Step" study was initiated with three teams at Princeton, General Atomic, and Oak Ridge National Laboratory. It continued for 2 years and was judged inadequate because not enough engineering data would be developed. When the Princeton Large Torus achieved "Reactor Relevant" temperatures in 1978, confidence in the ability to actually build a fusion power plant rose tremendously. An Engineering Test Facility study was launched to demonstrate the performance of the high technology elements that will make up a fusion based power system. The confidence was widespread. The House Science & Technology Committee and, specifically, the Energy Research & Production Subcommittee understood the great potential and provided effective leadership in initiating and enacting Public Law 96-386. Known as the Magnetic Fusion Energy Engineering Act of 1980, it called for the design of a Fusion Engineering Device and movement of the program into the industrial sector through a Center for Fusion Engineering. McDonnell Douglas and other companies competed for participation in the Fusion Engineering Design Center established at the Oak Ridge National Laboratory to begin implementing this government plan. Fusion was poised for the big push to accomplishment. The European consortium of countries and the Japanese nation were similarly united to develop the physics, master the technology and compete for the huge world market in the future fusion energy era.

By the end of 1982, Administration policies, priorities and funding dictated that the engineering phase of fusion development must be delayed and that program advances must rely on upgrading of existing facilities.

The tokamak concept was further confirmed by GA Technologies when they achieved a Reactor Relevant value of the meaningful Beta parameter in 1982. Then in 1983, the MIT Alcator C exceeded the Lawson criterion for the first time. This gave additional confidence in our understanding and ability to confine fusion reactor plasmas.

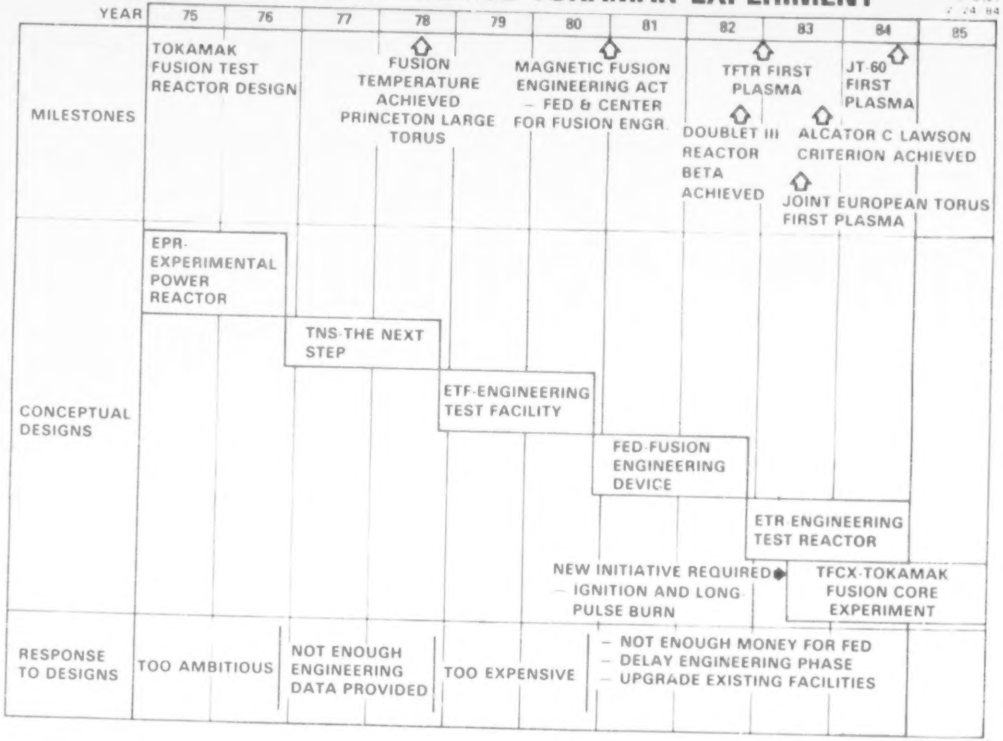
There are lessons to be learned from these experiences. Principally, we've seen that a multi-billion dollar project is not acceptable. The next device must have a Plant and Capital Equipment cost less than $1 billion. We've also learned that a new, large project cannot simply be added on top of a generic base program. A restructuring of Magnetic Fusion Energy priorities is necessary so that the lead project establishes requirements for the base program.

A design team centered at Princeton Plasma Physics Laboratory has now evolved a science based configuration which is currently called the Tokamak Fusion Core Experiment (TFCX). It is programmed to take advantage of new science from the Tokamak Fusion Test Reactor, the Joint European Torus, and the Japanese JT-60. It will also be able to employ new technology from many complementary experiments and tests in a timely manner. There is confidence that it can achieve the long time burn of a fusion plasma.

The fusion community has arrived at the concensus that the Tokamak Fusion Core Experiment is absolutely essential to the future of the fusion program. This is of primary importance because such agreement has not been achieved before. It demonstrates a change in priority away from the widespread Base Program. This will cause displacements, and possibly pain, to some who now endorse the shift of funding. A number of industrial organizations like McDonnell Douglas have 10 years of experience working with the fusion laboratories and universities and have developed competence to perform much of the TFCX work. Having industry in place is significant because the world competition to supply high technology products employing fusion technology will surely increase in the coming decades. Industrial organizations have supported the fusion program with tens of millions of dollars over the past decade because of its great promise. We feel it is necessary to stop this two-year cycle of definition, then rejection. The TFCX conceptual design should be started immediately by

Princeton Plasma Physics Laboratory with industrial teams selected through competition. A TFCX line item should be started in fiscal 1986 along with an increase in the total Magnetic Fusion Energy budget. A success-oriented schedule should be established by the Department of Energy to bring the TFCX into operation at the earliest possible date. By moving ahead with this opportunity, Congress and the Department of Energy will give industry the justification for continuing our investment of good men and resources. The knowledge which will come from fusion research and development will greatly benefit our country. We should move ahead smartly with the program.

DESIGN OF NEXT LEADING TOKAMAK EXPERIMENT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MAGNETIC FUSION ENGINEERING ACT – FED & CENTER FOR FUSION ENGR.
TFTR FIRST PLASMA
DOUBLET III REACTOR BETA ACHIEVED
JT-60 FIRST PLASMA
ALCATOR C LAWSON CRITERION ACHIEVED
JOINT EUROPEAN TORUS FIRST PLASMA
CONCEPTUAL DESIGNS
EPR-EXPERIMENTAL POWER REACTOR
TNS-THE NEXT STEP
ETF-ENGINEERING TEST FACILITY
FED-FUSION ENGINEERING DEVICE
ETR-ENGINEERING TEST REACTOR
NEW INITIATIVE REQUIRED
– IGNITION AND LONG-PULSE BURN
TFCX-TOKAMAK FUSION CORE EXPERIMENT
RESPONSE TO DESIGNS
TOO AMBITIOUS
NOT ENOUGH ENGINEERING DATA PROVIDED
TOO EXPENSIVE
– NOT ENOUGH MONEY FOR FED
– DELAY ENGINEERING PHASE
– UPGRADE EXISTING FACILITIES

MCDONNELL DOUGLAS
CORPORATION

Mrs. **Lloyd**. Thank you very much.

Dr. Shanny, please proceed.

STATEMENT OF DR. RAMY A. SHANNY, PRESIDENT, INESCO, INC.

Dr. **Shanny**. Thank you It is a pleasure to be in front of this committee. I am the chief executive officer of INESCO, which is headquartered in La Jolla, Calif. I will not read my statement if it can be submitted for the record.

Mrs. **Lloyd**. Without objection, all of your complete testimony will appear in the record.

Dr. **Shanny**. Thank you.

Since 1976, the inception of the company, we have pursued the approach of a compact Tokamak, basically. Since 1981, we have expended $15 million of private funds. We are not currently, and we haven't been funded by the DOE since 1978.

During that time, we have developed a consistent design for compact copper machines, all of which can achieve ignition and sustained burn for as long a period as one wishes to pay for the power supply and at burn levels of a few hundred MW. Those designs which have been to date completed includ the development of new alloys which previously were thought were impossible to achieve, to achieve very high magnetic fields, insullators which will allow you to operate in a near term radiation environment, all the fabrication techniques under new patents which were evolved, and the designs which were completely consistent to make devices which can achieve ignition and burn for continuous periods of a few hundred seconds.

In order to assist us as we have been sort of out of the community, we were fortunate enough to have enough courageous people to join a review committee, headed by Congressman McCormack, as he said, which gave us basically technical advice and said, you are missing that point or you are missing that point, which is being done in the community.

Mrs. **Lloyd**. I must compliment you on your selection.

Dr. **Shanny**. Thank you.

I want to stress the point that our committee is limited to providing us technical advice and nothing else.

We have been extremely careful in designing—the whole company, all the company does is fusion, basically, today. We have been extremely careful in doing an integrated design which would hold up if all the physics which is derived from the fusion community and the effort done by the government overseas and everywhere holds up to what they say should be the case.

We are today in a state where if we had the financial resources, we can within a period of 5 years from the day the check is printed attain the position where we have ignition and sustained burn. We do cost estimates based on industrial bases. We are quite efficient in our operation I believe. We estimate the cost of the project is $100 million for a test site and approximately $10 million per fusion machine

The reason is the fusion machined are relatively small. They cost between $5 and $10 million. They are in major radii between 70 to 90 centimeters or possibly a meter. The advantage of that approach is that you don't have one single machine which costs you a fortune which, if when you get to ignition and you start burning and you have a big accident, which you could have, you haven't lost an enormous capital investment.

I don't believe, personally, that is a personal opinion, that the physics is known to that extent that I can say, this is the ideal aspect ratio for machines. To prepare this way, I build five machines. My major cost has been in the capital investment in the plant. Tomorrow, somebody else may come up with another brilliant idea, and that site can accommodate it at minimum cost.

The main thing we have progressed in is essentially the development of those alloys. Years ago, when we were reviewed, the people were saying that you cannot attain those kinds of fields and you cannot fabricate those kinds of designs. We have improved the state of copper alloys by better than 50 percent. Today, the requirements for that copper alloy are now showing up from all laboratories for the utility in rail guns, which is a new military application and in other advanced technologies. This type of engineering development allows us, with great confidence today, to design the machine which we have designed and we believe we can build it.

When we estimate the cost, we don't estimate the cost by pounds; we estimate the cost by how many hours does it take the machine and how many times are you going to change the machine tooling. We are doing is specifically as a production.

We are basically seeking as close cooperation with the main line program as possible. We need their input.

As the chief executive officer of the company, I feel strongly that it is to the benefit of both the DOE and INESCO to work closely together and combine our technical resources toward the common national goal of fusion power at the earliest possible time. INESCO has expended a considerable amount of resources and is continuing to do so. However, I believe we have reached a degree of maturity both as a company and technically, and are willing to share our proprietary technology with the DOE and its institutions. I firmly believe that this cooperative effort will lead to significant progress in the national fusion program.

Now, if I may take, Madam Chairman, a second, I would like to make a comment in regard to a discussion which was previously initiated. It is with regard to a statement of Dr. Furth with respect to something that he says I believe, and I would like to clarify my position.

He stated that copper machines, as reactors, are not viable and I believe that way because they would not produce enough energy to compensate for the energy you consume in the magnets.

Let me say the following: Fusion is still a high risk business, whether INESCO's approach or the DOE's approach. To state today that the main line fusion approach to big superconducting reactors is viable either technically or commercially is a maybe. It is a high risk. To state conclusively that the copper machine cannot attain certain plasma efficiency which might not make it commercially attractive is also not a conclusive fact. You cannot say one way or another.

The tradeoff which is done with copper reactors, if they can be made to work, and that is only the physics will tell us that and you will find that when you sustain the reaction, is that you have done a tradeoff. Instead of doing the capital investment in a plant in a major fusion reactor machine which will cost $2 or $3 billion, and the rest the capital plant is smaller and the plant is beautiful and extremely efficient and nice. In our case, the fusion component is a minor part of the plant, may cost a few tens of millions, but the rest of the plant will be much more expensive, will have a lot of thermal power, the efficiency will be low, but the capital risk is much less.

At the bottom line, when I look at it from a corporate point of view is what does electricity cost and what is the risk factor for the corporation.

That is basically where I would like to end my statement. Thank you.

[The prepared statement of Dr. Shanny follows:]

TESTIMONY

of

DR. RAMY A. SHANNY

PRESIDENT, INESCO, INC.

before the

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

of the

HOUSE SCIENCE AND TECHNOLOGY COMMITTEE

Washington, D.C.
February 29, 1984

Testimony of Dr. Ramy A. Shanny
President, INESCO, Inc.
before the
House Science and Technology Committee
Subcommittee on Energy Research and Production
February 29, 1984

Mr. Chairman and Members of the Committee: It is an honor and a pleasure to appear before you today, as we last did two years ago. I am Dr. Ramy Shanny, President of INESCO, which is headquartered in La Jolla, California. Prior to joining INESCO, I was Director of the Plasma Physics Division of the Naval Research Lab. Today, I want to tell you of our work and accomplishments to date and the potential benefits from this work work for the DOE fusion program in its progress toward the attainment of ignition and controlled burn.

Since its inception in 1976, our Company has expended considerable sums in pursuit of our proprietary approach to small scale fusion tokamaks. Since 1981 alone we have expended approximately $15 mill on derived from private investment. In this effort, we have assembled a "critical mass" team of highly skilled engineers, to translate a conceptual idea into practical engineering designs.

INESCO has already designed a family of compact watercooled high strength copper allow tokamaks which should attain ignition, and allow for the study of controlled burn, at fusion power output in excess of a few hundred MWth. These designs are premised on the knowledge of plasma physics derived from the DOE and international tokamak research programs.

Toward its goal of demonstrating fusion in high field tokamaks, INESCO already has accomplished several significant materials engineering and design achievements:

Specifically, INESCO has developed special copper alloys for production of high magnetic fields in toroidal magnets. We have also developed inorganic insulators, which are radiation resistent, for use in these high field magnets. Each of these developments has advanced the state-of-the-art of water-cooled, high-field tokamaks.

In addition to our progress in materials development and engineering, INESCO has carried out the detailed design of a family of high-field, compact tokamak machines, ranging in size from a major radius of 70 centimeters to a size of about one meter. These are detailed engineering designs for experimental tokamaks which are ready for engineering component testing; these are not conceptual designs which will take many years to bring to completion.

INESCO's small tokamak designs are based upon knowledge of plasma confinement obtained from all relevant work in the large tokamak fusion research program of the Department of Energy and other tokamak programs through the world. All of our machine designs are able to reach fusion ignition conditions and sustained burn in a deuterium-tritium (D-T) plasma under the standard confinement scaling laws obtained in the DOE program. This sustained burn would generate several hundred MW of fusion power over a machine life of approximately 1000 hours. The length of burn time will be limited only by the capabilities of the driving electric power supplies and by whatever physics phenomena may appear to quench burning.

In addition, all of our machines have been designed so as to accommodate all of the diagnostic instrumentation needed for meaningful experiments and for plasma control, and all of the power supply and cooling systems needed to ensure successful operation.

The philosophy behind these designs has been to devise relatively inexpensive tokamak fusion machines over a range of configurations to allow experimental optimization of plasma performance by testing a number of such varied machines. Each of these tokamaks would cost approximately $10 million, depending on the specific size of the machine. It should be noted that in a program of this type the major costs will be construction of the test facility with its auxiliary equipment. This facility will serve as the central testing ground for the repetitive testing over a range of design parameters of a succession of inexpensive tokamak fusion devices. In its entirety, this test facility should cost no more than $100 million, and could employ upgraded existing DOE facilities for even further savings in time and cost. By this means all of the relevant physics of fusion ignition and burn can be explored at modest cost and in a relatively short time.

This is a classical engineering R&D approach to the acquisition of knowledge, and is reflected in the nature of our technical staff. We are primarily a company of talented nuclear, electrical, and mechanical engineers, with only a few plasma physicists. All of our physics is based on the know-how developed in the U.S. and international tokamak fusion programs, and we maintain limited informal interaction with some of the relevant DOE-supported tokamak fusion research laboratories.

To assist us in the review of our progress, evaluate and provide technical guidance, a Review Board was established under the auspices of Congressman M. McCormack which includes some of the leading authorities in the field, including those from the mainline fusion community (a complete list is attached). It is highly commendable that these leading members of the fusion committee are willing to contribute their technical knowledge and wisdom to a small fledgling company.

Two unique features have characterized our work. First, we early recognized that the success of high-field small machines would depend strongly on the attainment of magnet coil materials strong enough to support the high fields desired. To this end we undertook a major effort on the development of improved copper alloys of high strength and high electrical conductivity. This work has been amazingly successful. We believe the development of these materials will give INESCO the ability to achieve a burning plasma in a device that is a small fraction of the size, and the cost, of any other concept now being considered in the United States.

We now have a proprietary and patentable process for the manufacture of alloys superior to any others previously available from commercial suppliers, and these can be made in thousand pound quantities, not just in laboratory samples. While doing this we have also developed all of the bonding and welding techniques needed for fabrication of these alloys into coils and other structural shapes required by our machines. In addition, we have developed electrical insulator materials and application processes which exceed the requirements of machine operation.

The second important and unique feature of our work has been the development of a fully synergistic and integrated computerized systems design code for machine design over a wide range of parameters. It is the kind of detailed parametric design code traditionally used in the aerospace industry, and we believe it is the most advanced design tool of its type in the fusion field.

As you may know, the Soviet Union is working actively on a program that utilizes small copper coil tokamaks. The Soviet program is also an engineering design program that could lead to ignition within several years. However, the Soviet Union will not be able to achieve sustained burn. We believe that an adequately financed INESCO can sustain burn before the Soviets for two reasons: first, they have not developed the high conductivity high strength copper coils, insulator technology and fabrication techniques that INESCO now can produce; and second, they lack the overall infrastructure of the USA.

Thus, if INESCO's development program is not impeded or delayed, the U.S. is likely to achieve sustained burn before the Soviet Union. Moreover, we believe INESCO's contributions to high field tokamak engineering will be of significant value to the U.S. mainline program as it proceeds toward development and design of the world's first tokamak fusion reactor.

As CEO of INESCO, I feel strongly that it is now to the benefit of both DOE and INESCO to work closely together and combine our technical resources, toward our common and national goal of fusion power at the earliest possible time. INESCO has expended a considerable amount of resources and is continuing to do so. However, I believe we have reached a degree of maturity both as a company and technically, and we are willing to share our propriety technology with the DOE in cooperation with its institutions. I firmly believe this cooperative effort will lead to significant progress in the national fusion program.

Thank you.

INESCO Science and Technology Study Committee

Mr. Mike McCormack, Chairman
(Formerly, Member of Congress, former
Chairman, House Subcommittee on Energy
Research and Production)
President, McCormack Associates, Inc.

Dr. Melvin B. Gottlieb
Professor Emeritus
Princeton University

Dr. Robert Gross
Dean, School of Engineering and
Applied Science
Columbia University

Dr. Robert A. Krakowski
Group Leader, CTR-12 Division
Los Alamos National Laboratory

Mr. John W. Landis
Senior Vice President
Stone & Webster Engineering Corp.

Dr. Lawrence M. Lidsky
President, TFG, Inc.
(also Professor of Nuclear
Engineering, M.I.T.)

Dr. Ronald Parker
Professor of Electrical Engineering
M.I.T.

Dr. Marshall N. Rosenbluth
Director
Institute for Fusion Studies
University of Texas

Dr. Norman Rostoker
Professor of Physics
University of California, Irvine

Dr. Martin Summerfield
President
Princeton Comubstion Research
Laboratories, Inc.

Professor John Dawson
Director for the Center for Plasma
Physics & Fusion Engineering
Dept. of Physics
UCLA

Mrs. LLOYD. That is a very interesting observation, Dr. Shanny. Thank you for your testimony.

Dr. Dean, you stated in your testimony that you thought the engineering aspect of fusion development did require substantial additional effort now. What additional engineering work do you think we should be doing this year?

Dr. DEAN. I mentioned one thing which I think is relatively inexpensive and that is I believe that there should be more emphasis on reactor design studies. That is the primary area. I think that the entire TFCX effort, if it gets going, I would consider that to be part of what I would advocate as significant additional engineering effort. If they are going to build that machine, in addition to conceptual design which will involve some engineering, you will have to begin some technology component development and prototype design and testing program in order to be able to make that a reality.

So, I view, and you asked this question of Dr. Mense also, I believe that to embark on a TFCX type machine does put the program on an engineering and development path which would increase the engineering component of the program.

Mrs. LLOYD. Do you think we need to just do a study to determine what additional engineering facilities are needed?

Dr. DEAN. No. I think the Department of Energy has a very good program of identifying its R&D needs and its technology development facilities. There is a study going on at UCLA right now under Professor Abou to look at all the nuclear facilities that are needed to test out things in fusion and when that study is completed later this year, I would hope that funds would be provided to carry out some of those technology facilities in addition to maintaining the present facilities such as the Tritium Systems Test Facility at Los Alamos, and the Large Coil Project at Oak Ridge. There is also an RF development program that has been proposed at Oak Ridge for a test facility.

These things are important from a technological and engineering point of view. We, right now, use neutral beams to heat our plasmas and we know that we can't do that in reactors. We are planning to use RF heating and we need to develop those RF test facilities and components in order to be able to use them in machines like TFCX.

So, there is a whole host of small technology and component development needs. I think one of the things we worry about in this area is that it has been said that the development and technology program will be devoted to TFCX. That is fine if the programs are, in fact, engineering programs that support the TFCX. I think one of the worries is that they may take the money from those programs to construct the TFCX thereby cutting back those programs, and then that engineering technology might not get done.

Of course, we don't know that. It is just a fear. I think we need to recognize that if we are going ahead with the machines that do what TFCX is going to do, we need strong engineering technology development program.

Mrs. LLOYD. Thank you very much, Dr. Dean. I want you to know that this committee appreciates your testimony and your insight. We appreciate your being with us.

Mr. Motchan, excluding the drafting of conceptual drawings, what engineering work do you think we need to do this year that is not scheduled?

Mr. MOTCHAN. I would defer that question to my colleague, Dale DeFreece, who has been working that exact problem in detail.

Mr. DEFREECE. I am Dale DeFreece, manager of fusion at McDonnell Douglas.

I think what we need to do is move out in an aggressive way with TFCX. We know what the parameter space is. There has never been a better definition of what the basic parameter space is. There are a number of critical issues that need to be addressed this year in terms of the tradeoffs of just what this machine should be. A very aggressive approach toward understanding those issues and clarifying them should be done this year.

Mrs. LLOYD. Would you expound on those issues?

Mr. DEFREECE. Well, the kind of things that we enter into are—should it be superconducting or should it be copper? That is a very major issue as we go into this machine. Another one is, how will we heat the device? Should we rely upon RF totally in this machine, or should it also have neutral beams? Another would be the current drive issue. Can we depend upon RF for current drive or should we have sufficient volt seconds to drive the current that we think is necessary or should we depend on RF?

Those, I think, are the main issues. There are some other ancillary things that will have to be addressed, but they probably come on a little later as we get through these first wickets.

Mrs. LLOYD. I certainly went over our time today, but I appreciate your being with us. If you have additional insight which you would like to share by correspondence, we certainly would like to have your input as well.

Thank you.

Dr. Shanny, we are happy to have you with us also. In your written testimony, you mention development of special copper alloys. You also mention in your testimony toroidal magnets. Have you made any magnets?

Dr. SHANNY. We have made components for magnets. The problem we have as a private company is that we have been trying to hold a team together in order to survive basically and progress as well as we can. We have designed them in detail. We made big copper chunks and tested them and fabricated them, but not—to make magnets would cost an additional $1 million a year which is out of pocket. That means I have to make a tradeoff between if I lay off some people or don't pay my rent or build magnets.

Mrs. LLOYD. You have some priorities too, then.

Dr. SHANNY. I have priorities, as well. We are trying to generate auxilliary business right now that will, hopefully, with the income from that, allow us to do some of those things in the future. But the small increment in funds we can basically move ahead to testing components. That is our plan, actually, for the next year, is to test the components that we have designed. We have designed everything to operate in a nuclear environment. It was done as a total engineering thing with minimum emphasis on discovering—I don't allow physics research. That has to come from the main line program, but the engineering is all done in-house and very carefully.

Mrs. **Lloyd**. What are the reactor conditions that you hope to achieve?

Dr. **Shanny**. For the experimental stage, we hope to achieve—the machine is designed to sustain burns at 300 MW of steady burn. The difference between us, basically, and the current proposed TFCX approach is in the plasma density.

Because our magnetic fields are substantially in excess of the ones proposed for TFCX, we can operate at an order of magnitude more density. As such, to study the same confinement stability, we can go for much shorter times, because our tau is the product of the two.

So, the machine has been designed as if there are reactors at that power level, not to be self-sufficient in energy, but to put out the neutrons at 300 MW flux, and it can be operated if you have with sufficient volt-seconds in the machine if it is burning for about 200 seconds, and the machine can keep running and which will not be damaged by any irradiation for at least the lifetime of an experiment.

It has also been designed to withstand 300 full disruptions at full burn conditions, which is a very difficult requirement, but we take the view that if we lose a machine in the middle of a burn because of a terrible disruption under most pessimistic conditions, we will junk that machine. For $5 million, I would rather not build the remote equipment to handle it. I would rather dump the cell and go to the next one.

Mrs. **Lloyd**. That is a very interesting perspective.

Dr. **Shanny**. We do two cells at a time, so you have two machines operating simultaneously, and if one of them breaks or I have to repair, I don't have to maintain the overhead of the whole facility and the capital depreciation of the facility. I have another machine which is operating. The plan is to build five machines, basically, of different configurations.

Mrs. **Lloyd**. Have the Soviets achieved a sustained burn with their small coil?

Dr. **Shanny**. No; they are trying to. Conversations I have had with Mr. Velikov and others, he claims every year, I will beat you and I will get there a year before. They do not have the technology to do the same—what they can do is they can do a pulsed experiment. It means "show that I have got an ignition." They cannot burn.

They don't have the technological infrastructure that we have in this country. If I have a problem, I pick up the phone and I say, Union Carbide, do this for me. They don't have that. They will have to build a plant to do those insulators, and we have that.

Mrs. **Lloyd**. Thank you very much. It is very interesting.

I thank all of you. It has been a very enlightening afternoon. I think everyone in the audience shares my feelings.

We do have one other witness at this time. We certainly welcome you, too, and we appreciate your patience for waiting all afternoon as well. We have two witnesses scheduled, but Mr. Maglich is not here, I understand. Ms. Marsha Freeman of the Fusion Energy Foundation is with us. Ms. Freeman, we certainly welcome you here this afternoon. I don't believe you have appeared before this subcommittee in the past. We are happy to have you.

Please proceed.

STATEMENT OF MS. MARSHA FREEMAN, FUSION ENERGY FOUNDATION

Ms. **Freeman**. Thank you very much. Thank you for the opportunity to testify, Madam Chairman and members of the subcommittee.

I would like to, I guess, end this afternoon's session by approaching the fusion program from two, I think, somewhat different angles than most people have discussed. One is from the economic side and also from the national defense side.

The testimony that was presented before this subcommittee last week by the Department proposed a go-slow fusion program that we believe is scientifically incompetent compared to what the fusion program has to accomplish.

The fronties of plama physics research are of immediate importance for the solution to the world's severe energy deficit. These plasma physics research questions also overlap with the most crucial questions of directed energy beam weapon defense and with the frontiers of astrophysics and bioengineering research. The pace and level of fusion funding must be considered within this framework of moving the Nation forward in the frontiers of science for economic growth and national defense.

In Dr. Trivelpiece's testimony before this committee last Thursday, he mentioned that in the current climate with the price of oil and yellow cake both going down, that it would be difficult to make a rational case for accelerating the fusion program. Under questioning from members of the subcommittee, he did agree that the goal of demonstrating the commercial viability of fusion could be accelerated with increased funding, but, as was brought up this afternoon as well, that high budget deficits were making funding increases difficult.

I think it is very important to understand the economic impact of the money that was spent in the fusion program and also what is going on in the economy more generally. One of the major points made by the administration in not increasing fusion and other funding, I would include in this the civilian space program, advanced nuclear energy, and other areas, is that we have to hold the budget deficit down.

This is based on the premise that if we can do that, we will have a continued economic recovery, and to have this economic recovery, a reduced deficit is necessary and prerequisite. I would maintain to you, and our foundation has done studies of this looking at it in detail, that there is no economic recovery that is going on at this time, and that all of the really general parameters of economic growth have shown a contraction in productive jobs, energy consumption has fallen in the last 2 years for the first time since the introduction of electricity 100 years ago.

Therefore, to say that our continued economic vitality is dependent upon lowering the deficit when that economic vitality doesn't exist, I think, is somewhat of a very strange argument. In terms of actually getting to a state of economic growth where we see the consumption of electricity going up again on a positive rise, we have very strongly held that it is actually the rate of introduction of new technology that provides economic growth. It is not deficits or interest rates or the money supply or many of the other superstructural things that people consider.

The economy of the United States grows when science and technology are bringing new things into industry. The greatest period of economic growth we have seen since the war was actually the result of the spending in the Apollo program and the civilian space program.

We feel that the fusion program spending in terms of economics is crucial. You could even look at the fusion laboratories, our major national laboratories as laboratories for new industrial processes, low and high temperature plasma processes for materials, synthetic fuels, chemistry, steelmaking. As Al Mense said, it is an endless list of applications that could be made for fusion, some of which may be more immediately important than even electricity.

Therefore, I think we should see our fusion research as bringing us into this forefront of the plasma age and a whole new array of industrial technologies.

It is also, I think, very important to look at the funding for fusion. It was compared earlier today to the other funding for programs in the Office of Energy Research, high energy physics, et cetera. I have included a chart at the end of my testimony which also includes a comparison of this very small increase in fusion funding that the administration is requesting to increases they are requesting in other areas, and I think it is very astonishing. Compared to a 3-percent increase in fusion, you have 100 percent increases in conservation, solar, and other areas.

So, it is just simply not the case that the administration is not willing to increase funding in all of energy. I feel that the priorities are completely standing on their heads at this point.

The second and really final point I wanted to make is on the question of national defense. Last March 23, President Reagan announced a program to develop technologies as he put it, to render nuclear weapons impotent and obsolete. This kind of directed energy beam weapon defense effort is going to require conquering very challenging questions in both science and technology.

The kind of work that is done in plasma physics and other scientific areas in fusion and definitely in the technologies of cryogenics, magnets, accelerator technology, and many, many others are important both for fusion and also will have very important benefits and transfers of technology into this very important defense program.

We, of course, at the Fusion Energy Foundation, are not promoting any one program or any one design of a tokamak or any other system. What we are most concerned about is that the commitment that was made 3 years ago by this Congress for the Magnetic Fusion Energy Engineering Act actually be accomplished. The funding level that was suggested at that time in 1980 has still not been attained today, 4 years since Mike McCormack introduced the legislation into the Congress.

I don't believe that the talent that we have in the scientific community is going to stay with a program that has no goals and no vision for the future. Again, I think in terms of both its potential economic impact and questions of defense, that we have to make this fusion program a national priority.

Thank you very much.

[The prepared statement of Ms. Freeman follows:]

FUSION
ENERGY
FOUNDATION

Box 1438 • Radio City Station • New York, N.Y. 10101 • (212) 247-8439

Statement of Marsha Freeman

Fusion Energy Foundation
New York City

before the
Subcommittee on Energy Research and Production
of the
House Science and Technology Committee

February 29, 1984

Madam Chairman and Members of the Subcommittee:

Thank you for this opportunity to present the views of the Fusion Energy Foundation on the FY85 budget request for the Department of Energy Magnetic Fusion Energy program.

The testimony that was presented before this subcommittee last week by the Department of Energy proposed a go-slow fusion program that is scientifically incompetant compared to what the fusion program must accomplish.

The frontiers of plasma physics research are of immediate importance for the solution to the world's severe energy deficit; they directly overlap with the most crucial questions of directed energy beam weapon defense and with the frontiers of astrophysics and bioengineering research. The pace and level of fusion funding must be considered within this framework of moving the nation foward in the frontiers of science, for economic growth and national defense.

In the three years since the passage of the Magnetic Fusion Energy Engineering Act of 1980, the Reagan Administration has refused to fulfill the mandate of that law with regard to both the proposed funding profile for fusion and a program plan to meet the milestones laid out by the Congress.

In his testimony representing the view of the Administration before this Subcommittee on February 23, Dr. Alvin Trivelpiece stated that because of the "current climate" where the prices of oil and yellow cake are going down, "it is difficult to make a rational case for accelerating the [fusion] program." Under questioning, Dr. Trivelpiece agreed that the goal of demonstrating the commercial viability of fusion could be accelerated with increased funding, but that high budget deficits make funding increases difficult.

The Administration has continued to try to convince the American people that the so-far invisible economic "recovery" depends upon decreasing the budget deficit. First, it is important to understand that there is no economic recovery. All real production indicators show that basic industry, farming, and manufacturing have been contracting, and that the number of real productive jobs in the economy is shrinking. Only the made-up figures of the Federal Reserve indicate that there is a "recovery."

Second, the simple amount of the federal budget deficit has nothing to do with the state of the economy. The only determining factor is the mix of activities the government is engaging in which are causing the deficit. If the government were using this credit to expand industry, develop new energy and industrial technologies, expand the civilian space program and push forward the laser, particle beam, and plasma technologies required for new defensive antimissile systems, there would be economic growth as a result of the budget deficit. If the millions of currently unemployed Americans were working and paying taxes, budget balancers would not even have a budget deficit to worry about.

Third, the only basis for a real economic recovery is the priority investment in the frontiers of science and technology --fusion and directed energy defensive weapons--for this will ensure a continuing increase in productivity by the introduction of new technology.

Cutting crucial R&D programs, using the excuse that there must be a cutback in government spending, ensures there will be a slowdown in the rate of introduction of new technology into the economy, which is, after all, the driving force of increased productivity and economic growth.

The relationship between the rate of introduction of new technology into basic industry and economic growth was demonstrated in a study done last year by the economic staff of the weekly magazine Executive Intelligence Review and the technical staff of the Fusion Energy Foundation. This groundbreaking study, using the LaRouche-Riemann economic model, showed that a crash program for directed energy beam defense development could add millions of new highly skilled jobs to the U.S. economy, raise per capita income by 5 percent per year, eliminate the trade deficit within two years by gearing up exports, and produce a 25 percent per annum rate of growth in the economy. This effect would be caused by the transfer of technologies developed for the military into the industrial economy.

The potential economic impact from advanced energy technologies such as fusion, is even greater. Fusion research should be seen as a national laboratory for the new industries of the economy. Plasma-based steelmaking, chemistry, materials processing, and even space travel will revolutionize the U.S. economy. The nation's fusion scientists should have the resources to begin now to prepare the technologies of the plasma age.

Regardless of how depressed the electric utility industry may be at the present time, if there were even a modest upturn in economic activity, there would be shortages of on-line electric generating capacity in less than five years. According to the National Electric Reliability Council, certain regions of the country could experience brownouts by 1985. To plan for the future on the basis of the fall in electricity demand for the last two years, is to state categorically that we will be in a depression for years into the future.

The Administration has stressed in its statements on science and technology policy that international cooperation must bail the United States out of tight R&D budgets. It has even been suggested that national sovereignty might have to be compromised as other nations are encorgaed to build the fusion facilities and perhaps other facilities that the U.S. niggardly refuses to spend the money on.

Rather than taking on the responsibility that the United States has, as the world leader in science and technology, to lead the international effort in fusion, Administration officials are questioning whether or not to relinquish that role.

Little consideration has been given to the fact that the frontier scientific questions in fusion are also crucial for national defense. The defensive technology thrust announced by President Reagan last March 23 will require the creative input from fusion scientists. As Dr. Edward Teller has stated, it is time for the fission and fusion communities to reunite to tackle and solve these challenging problems, to protect this nation and allies from nuclear attack. We cannot slow down fusion research and development without effecting national defense.

For these economic and military reasons, the U.S. fusion effort must be "technology limited" and not "funding limited." The funding profile that was the foundation for the Fusion Act four years ago, has still not be achieved today. The FY85 budget request from the Department of Energy is still below the $500 million mark that was deemed necessary at the beginning of 1980.

The funding level for the magnetic fusion program should be determined by how quickly money could be absorbed for the design, construction, and operation of the Tokamak Fusion Core Experiment; the continued use and upgrade of other facilities and experiments; and the expansion of physics research which will provide new ideas on better fusion machines in the future.

There must be room in the budget to investigate the possibility of using polarized fuels to relax the classicial parameters for fusion ignition. Promising concepts, such as the plasma focus, which have had to rely on other government agencies for support, should be brought directly into the fusion program. And serious attention should be given to the role that our fusion program must play in the directed energy beam defense effort.

Any society which develops policies based on the notion that science and technology programs are a luxury which the country may not be able to "afford," or that these programs will bring to fruition a technology that "we do not need right now" has lost any sense of history and certainly any vision for the future.

I would be pleased to answer any questions. Thank you.

The Fusion Energy Foundation is a non-profit organization with over 15,000 members in the United States, and is the publisher of FUSION Magazine. FUSION Magazine, which is published in six languages, has a circulation of over 250,000, and has played an important role in helping to forumlate fusion policy for the past decade.

Marsha Freeman is the Washington editor of FUSION magazine, and the Director of Industrial Research for the Fusion Energy Foundation.

Ms. **Lloyd.** Thank you very much for your very fine statement. I certainly appreciate your testimony today.

Other than adding more money to the fiscal year 1985 budget, can you make other suggestions how DOE might maintain a strong, aggressive fusion program?

Ms. **Freeman.** I think that, as has been mentioned today, there are other concepts in addition to the two main line, mirror and tokamak, programs which have not necessarily been given the kind of support that they could. I don't think it is possible to do that unless the main line programs are assured of an adequate funding level and a really aggressive push, where the program, to quote Bob Hirsch, is technology limited and not funding limited.

So I think in a very real sense the money must come first in terms of then seeing how the DOE program could be broadened, how other promising experiments could also be accelerated, how others that are not perhaps getting very much support could get more Government funding.

I think that the other thing that has been lacking in the way the Department has looked at the fusion program, and again I think this is very strongly due to financial constraints, are in the other applications area besides electricity. I think if we did plasma materials processing, synthetic fuel production, and many other things, again, as I have said before, I think these areas might very well be very critically important to this country, even before we have run out of our nuclear and other technologies to produce electricity. So I think broadening the program in the applications area would be extremely important, as well.

Mrs. **Lloyd.** Thank you very much.

Ms. **Freeman.** You are very welcome.

Mrs. **Lloyd.** If there is no further business, again I thank everyone who was a witness today and all of you that are a part of the audience as well.

If there is no further business, the subcommittee stands adjourned.

[Whereupon, at 4:15 p.m., the subcommittee adjourned, to reconvene subject to the call of the Chair.]

APPENDIX I

ADDITIONAL STATEMENTS SUBMITTED FOR THE RECORD

.kms
fusion
inc. A Subsidiary of KMS Industries, Inc.
505 Second St., N.E., Washington, D.C. 20002 202/544-4280

JAN 17 1984

January 13, 1984

Honorable Marilyn Lloyd
Chairman, Subcommittee on
Energy Research and Production
B374 Rayburn Building
Washington, D.C. 20515

Dear Madame Chairman:

I am writing to request the opportunity for our company to appear before your Subcommittee as it examines the FY85 budget. KMS Fusion is a private company, which has been active in inertial fusion research since the inception of the program. We would like to present our views on inertial fusion. As the only private participant in the inertial fusion program, we feel that our company's views may be of value to you in framing this year's budget.

Our president, Dr. Alexander J. Glass, is prepared to appear at the convenience of the subcommittee. Please contact me at the above address or at 544-4280 if your staff would like to discuss scheduling or any other details.

I look forward to hearing from you.

Sincerely,

Terence C Liddy

Tererce C. Liddy
Senior Vice President

STATEMENT ON INERTIAL FUSION RESEARCH

by

Alexander J. Glass
President, KMS Fusion, Inc.

Submitted to the House Science and Technology Subcommittee
on Energy Research and Production
February 29, 1984

SUMMARY

The 1985 Administration budget calls for a reduction in funding for all elements of the Inertial Confinement Fusion program. We feel that with major new facilities starting operation, and critical experiments scheduled in the next few years, these budget reductions are counter-productive, and will slow down the pace at which vital information will be obtained. We request an increase in funding for KMS Fusion, to the level of $14 million in FY 85 operating funds. This figure is less than the amount authorized for 1984. This modest increase will enable us to accelerate our efforts to acquire both the physics data base and the requisite target fabrication technology needed to make more cost effective use of the major facilities available to the ICF program.

The Inertial Fusion Program

Fusion is the process which generates the sun's radiant energy, and which gives the hydrogen bomb its awesome power. In fusion, the fuel, consisting of the heavy isotopes of hydrogen, called deuterium and tritium, is heated to a temperature of 100 million degrees, hotter than the sun's interior. At this temperature, the hydrogen nuclei fuse together to form helium, with an attendant release of energy. The fuel must be confined, i.e., held together long enough so that the energy released exceeds the energy invested to heat the fuel to the reaction temperature.

Inertial Confinement Fusion (ICF) is one approach to confining and heating fusion fuel. Energy from a powerful laser or particle beam is concentrated on a spherical shell containing the fuel. The fuel is heated and compressed in the process, until thermonuclear conditions are achieved at the center, igniting the fusion reaction. The surrounding compressed fuel is then ignited by a propagating thermonuclear burn wave, releasing energy from the fusion reaction.

Inertial Fusion research has been supported by the Congress for over a decade, with two objectives. In the near term, the ICF program provides information about thermonuclear physics which is of vital importance to the nuclear weapons program. In the long term, ICF is viewed as an alternative path to harnessing fusion as a source of energy. In the Department of Energy, the ICF program is administered by the Office of Inertial Fusion, under the overall direction of the Assistant Secretary for Defense Programs.

For the past five years, three major facilities have been under construction in the ICF program. Each represents a different technological approach to driving the inertial fusion process. The **Antares** laser, a 40 kilojoule, carbon dioxide laser at the Los Alamos National Laboratory, started operation in 1984. **Nova**, a 100 kilojoule glass laser, will commence operation at the Lawrence Livermore National Laboratory early in 1985. **PBFA-II**, a light-ion beam facility at the Sandia National Laboratory, is scheduled to start operation in 1986. These facilities, along with other activities in the ICF program, will provide the information needed to assess the scientific feasibility of inertial fusion late in this decade. The Department of Energy has scheduled a critical review of the ICF program in 1986-87.

The Nova Project

In 1979, the Department of Energy approved the construction of the Nova laser at LLNL. Nova was originally conceived as a 20-beam, neodymium-glass laser, capable of generating 200-300 kilojoules of infrared light in three-billionths of a second (3 nanoseconds). However, DOE only approved construction of a ten-beam system, designed to produce 100 kilojoules of infrared light, but with frequency conversion capability, to convert the output to visible light. The total project cost for Nova is approximately $176 Million.

There are two basic approaches to driving an inertial fusion target. The laser light can impinge directly on the target (direct drive), or can be converted to x-rays in an enclosure ("hohlraum") containing the fuel mass, and the x-rays used to drive the target (radiation drive). Radiation-driven targets offer better uniformity of illumination than direct-drive targets, but sacrifice efficiency at lower laser energies. Both approaches are currently under investigation in the ICF program. Nova experiments will emphasize the radiation-drive approach.

In order to ignite the fusion reaction, it is necessary to heat the fuel to 100 million degrees, and to compress it to 1000 times the density of liquid hydrogen (1000 x L.D.). Nova experiments are unlikely to achieve both conditions simultaneously. However, it is expected that targets will be compressed to 500 x L.D. in the first year of Nova's operation, at a temperature lower than that required for ignition. Subsequent Nova experiments are expected to achieve 1000 x L.D., at sub-ignition temperatures. Data from the Nova experiments, taken together with information obtained from other sources in the program, will define the driver and target requirements for both ignition and high-gain targets. These experiments will thus provide critical and valuable data for the ICF program, data which will provide the basis for evaluating the applicability of ICF to both its military and energy objectives.

The Role of KMS Fusion

KMS Fusion is a private company, located in Ann Arbor, Michigan, and is the only private participant in the ICF program. In 1979, KMS Fusion was designated as a support laboratory for glass laser experiments. Its efforts are closely coordinated with the program of the lead laboratory for glass laser experiments, LLNL. Cooperation among the laboratories in the program is fostered under this arrangement. Since 1980, in recognition of the high value of the Nova experiments, KMS Fusion has directed its efforts to support the Nova objectives.

KMS Fusion has played a major role in the development of inertial fusion. The company began fusion research in 1971. Ten years ago, the world's first successful demonstration of laser-produced thermonuclear reactions took place at KMS Fusion's laboratory, in Ann Arbor, Michigan. Since then, the company has contributed many other innovative "firsts" to the program. KMS Fusion was the first to carry out experiments with short wavelength light, a technique which has been subsequently adopted world-wide. The company pioneered the use of frozen (cryogenic) fuel in ICF targets, a complex technique generally acknowledged to lead to improved target performance. Until 1983, when LLNL conducted its first cryogenic target series, KMS Fusion was the only laboratory in the world to carry out crogenic experiments. KMSF scientists had demonstrated, in 1981, that cryogenic targets perform better than gas-filled targets. Most recently, in 1983, the company developed a unique optical probe for visualizing the evolution of material above the surface of a target, a multi-frame holographic interferometer. This instrument is capable of providing stop-motion images in a fifty-billionth of a second. Up to four such images can be recorded during each experiment, which lasts less than a billionth of a second.

The company is a major supplier of targets and target components within the ICF program. The target fabrication effort comprises roughly half of the company's activities. This effort supports the internal research program at KMS Fusion, and also produces target materials and components for the other participants in the program. Several European laboratories also buy the sub-millimeter-sized glass Microshell® fuel containers that KMS Fusion has developed for fusion use. In addition to target fabrication, the company is pursuing long-term research to develop the large targets needed for the critical experiments to be performed on Nova. These targets involve several techniques and technologies developed at KMS Fusion, including cryogenic fuel, and various new target materials.

In addition to the company's efforts in materials science and target fabrication, KMS Fusion operates a highly productive experimental facility, the Chroma laser. Chroma is a two-beam laser, capable of producing up to 800 Joules of infrared light in a pulse of one billionth of a second duration. This energy can be efficiently converted to visible light, which couples more effectively to the fusion target. The laser is equipped with a pulse stacker, which allows us to synthesize pulses of arbitrary temporal shape, an important parameter for optimal target compression.

The Chroma laser is outfitted with two experimental areas. This allows us to carry out one set of experiments, while setting up another experimental configuration. At present, one experimental area is devoted to a unique optical system, devised and patented by KMS Fusion, which enables us to illuminate a target uniformly from all directions, with the light from the two beams of Chroma. Either gas-filled or cryogenic targets can be imploded in this target chamber, with either infrared or visible light. The second experimental area is used to explore the physics of radiation-driven targets. KMS Fusion is the only laboratory in the U.S. today actively exploring both directly driven and radiation-driven laser fusion targets.

Since 1980, the company has placed heavy emphasis on improving and developing its capability to detect and measure the complicated phenomena which occur within a fusion target. In any given experiment, it is necessary to measure effects on a spatial scale of hundredths of a millimeter, and a time scale of hundred-billionths of a second. The quantities measured include the density of the rapidly moving material vaporized by the incident laser energy, and the spectra of electrons, neutrons, ions, and x-rays over a wide range of energies. Today KMS Fusion has acquired the capability to measure all these quantities, simultaneously, with high accuracy. In developing our diagnostic capabilities, we have worked closely with the other laboratories in the program, especially LLNL, and gratefully acknowledge their cooperation and support.

By virtue of this increased diagnostic capability, and the flexibility and high data rate of the Chroma laser facility, KMS Fusion has become the principal source of information concerning the basic physics of radiation-driven targets in the ICF program. Over the past three years, we have conducted well over 1000 target shots on both radiation-driven and directly driven targets. These experiments have yielded basic and important information concerning the absorption of laser light by the target, the transport of energy within the target, and factors influencing the phenomenon of suprathermal electron generation, which limits target performance. These experiments have supplied data of vital importance to experiments performed on the Novette laser at LLNL, and scheduled to be conducted on Nova. A high degree of technical cooperation exists between the scientific staffs of the two laboratories, and the experimental results obtained at KMS Fusion are readily and quickly shared with the LLNL scientists as well as throughout the program. Both in target fabrication and in our experimental program, we are attempting to make the most cost-effective use of our facilities to support the near-term objective of the national program, the achievement of near-ignition conditions on Nova.

Technology Spin-Off

As the only private participant in the ICF program, KMS Fusion is the primary agent for the transfer of technology to commercial applications. Currently, KMS Fusion is marketing three commercial products which are based on technology developed within the ICF program. We have modified the tiny glass Microshell® containers, used to encapsulate fusion fuel, and have developed them for use as microcarriers for cell culture. Living cells, which produce biological materials such as vaccines, hormones, or interferon, can be grown *in vitro* on the surface of these microcarriers. Initial tests of our microcarriers have proven successful, and we are scaling up our production capability to commercial volumes.

In conjunction with scientists at LANL, we have also adapted our microshell® fuel containers for use as shockwave detectors. The tiny glass shells are filled with argon gas, and attached to the end of an optical fiber. An incident shock or pressure wave crushes the shell, causing the gas to emit a brief, intense pulse of light. This "optical pin" is currently in use in several laboratories studying shock and pressure phenomena, including LANL, KMS Fusion and other industrial laboratories.

Another product developed under the ICF program is the video multiplexer, a device whch allows us to connect several video graphics terminals to a single hard-copy printer. Commercial sales of this product began early in 1984.

The availability of the facilities developed in the ICF program has provided a powerful stimulus for the investigation of x-ray lasers, both at LLNL and KMS Fusion. In 1983 and 1984, two experimental campaigns were conducted at the Chroma laser facility, involving KMS Fusion scientists working with researchers from LLNL, NRL, and Imperial College in London, England. These experiments were supported by a subcontract with LLNL, and have provided critically important spectroscopic data to guide the choice of experimental materials used in x-ray laser experiments conducted on Novette, and scheduled for the Nova facility. These investigations would not have been possible, were it not for the existing capabilities at KMS Fusion in target fabrication, diagnostic instrumentation, data analysis, and theory, as well as a high data-rate, reliable experimental facility, all of which were developed under the sponsorship of the ICF program, and company funding.

1985 Budget Request

In 1983, KMS Fusion received $10.5 Million in operating funds, approximately 7.5 percent of the program total. In 1984, operating funds for KMS Fusion were authorized at a level of $14.5 Million, but not fully appropriated. Our operating budget in 1984 is $10.8 Million, again approximately 7.5 percent of the program total. In 1985, we are requesting an increase in operating funds to the level of $14 Million, less than the amount authorized in 1984. As we stated last year before this committee, we feel that the moderate expansion of our program made possible by this budget increment will enable us to make far more effective use of our existing facilities and staff, in support of the objectives of the national ICF program, and in particular, will lead to more cost-effective use of the $176 Million Nova facility.

With added funding, we will increase our efforts in the development of critical target fabrication technologies. In the forthcoming Nova experiments, target performance will be limited by the ability to fabricate complex targets to the required specifications. We feel that this topic has not received adequate emphasis to date. Improving target performance through advances in target fabrication technology leads to more cost effective use of existing facilities. We will expand the development of polymeric materials for large fuel containers. These materials need to be strong, capable of holding fuel, and physically uniform. We need to accelerate the development of diagnostic gas fills for targets, a technique which provides characteristic signals from within the target to measure the temperature and pressure achieved in the implosion. Given our extensive experience in cryogenics, we feel it is urgent to accelerate the development of large cryogenic targets, along with techniques for characterizing the condition of the frozen fuel within the target. We have recently made the first demonstration of the formation of a homogeneous and uniform layer of liquid fuel in a large, cryogenic target. This technology is critical in the design of high-performance targets for Nova.

We cannot carry out the necessary expansion of these activities by reducing other efforts in target fabrication. We must continue to support the ongoing experimental programs in our own and other laboratories, because these are the source of the vital physics data needed to design high-performance targets. With added funding, we shall accelerate our own experimental program, with first priority given to maintaining a full schedule of experiments on radiation-driven targets. We need to expand our capability to analyze the experimental data we are generating. Both added staff and equipment are needed. Furthermore, we need to accelerate the investigation of directly-driven targets. KMS Fusion has a unique capability to carry out these experiments. Direct-drive targets may provide the best way to obtain data on ignition requirements and high density implosions, at limited laser energy. These experiments benefit from the ease of diagnostic access, and allow the early development and test of cryogenic targets, as our recent experiments have shown. Direct-drive experiments also provide an excellent test bed for physical measurements and comparison to theory, since the geometry of these experiments is essentially one-dimensional

The University of Rochester operates the 24-beam **Omega** laser, which generates approximately 3 kilojoules of infrared light. To make the most cost-effective use of this facility, it should be converted to short wavelength operation, and equipped with cryogenic target capability. With added funding, KMS Fusion proposes to initiate the design and construction of a cryogenic target chamber for the Omega laser. With frequency conversion and cryogenic capabilities, it should be possible to carry out high-density target experiments, using Omega. These experiments would complement the direct-drive cryogenic experiments scheduled to be carried out at KMS Fusion late in 1984.

With added funding, we will also be able to accelerate the development of other required technologies. Diagnostic instrumentation remains an essential, and critical requirement, if we are to understand the complex phenomena which occur in a laser fusion experiment. We will develop diagnostic instrumentation on our facility, and transfer it to experiments at LLNL and elsewhere. We will also accelerate our investigations of new, cost-effective driver technology.

We are requesting a modest increase in funding for KMS Fusion in 1985, less than the amount authorized in 1984. This increment will enable us to accelerate our efforts in support of the major objectives of the national program, and will result in higher productivity in our own laboratory, and more cost-effective and timely use of the major facilities currently existing within the ICF program.

UNITED SCIENCES, INC.

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April 16, 1984

Mrs. Nancy Jefery
Subcommittee Energy Research & Production
B374 Raymurn Blvd
U.S. House of Representatives
Washington, D.C. 20515

Dear Mrs. Jefery:

Please find enclosed a copy of the Testimony of Dr. Bogdan C. Maglich, Chairman of United Sciences Inc., to be include congressional records as per conversation with Dr. Dugan.

Dr Maglich receive your letter on the very day of the hearing, and therefore, could not attend.

Sincerely yours,

Bogdan C Maglich

Bogdan C. Maglich
Chairman

BCM/jb

3/15/84

DEVELOPMENT OF 'ANEUTRONIC' NUCLEAR ENERGY

TESTIMONY
OF

DR. BOGDAN C. MAGLICH, CHAIRMAN OF UNITED SCIENCES INC.
(FORMERLY FUSION ENERGY CORP/ANEUTRONIX INC.)
AT THE HEARING OF

Science and Technology Committee of the U.S. House of Representatives on March 15, 1984.

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The Report on the "Energy & Water Development Appropriation Bill, 1984" of June 16, 1983 states:

> "To date, basic research in the fields of nuclear fission and fusion has largely overlooked the potential for aneutronic nuclear alternatives using light metals such as lithium, which produce no radioactive side-effects. The Committee recommends the Department give higher priority to this nonradioactive and nonproliferatory nuclear potential. The Department should allocate funds necessary to conduct a feasibility study to test the conditions necessary to produce a prototype aneutronic reactor, and to fully examine the theoretical principles of self-sustaining aneutronic energy." (98th congress, Senate Rep. No. 98-153, June 16, 1983).

At the Appropriation Conference, held on June 29, 1983 the following provision was agreed upon:

> "The Conferees agree that the administration shall consider including funding in the FY 1985 budget submission for aneutronic fusion activities, for consideration by the Congress." (98th Congress, H.R. Report No. 98-272, June 23, 1983).

On July 19, 1983, Dr. Alvin W. Trivelpiece, Director of the Office of Energy Research, U.S. Department of Energy wrote to me:

"The Department of Energy intends to make every effort to follow the conferees' suggested course of action and to give due consideration to future funding of aneutronic energy, based upon an evaluation of all the available evidence".

However, the DOE 1985 budget does not allocate a single dollar to aneutronic energy research.

The DOE's development of new nuclear energy sources is totally commited to demonstrating the highly neutronic DT fusion with one technology, the TOKAMAK. (D and T denote the fuel, a mixture of deuterium and tritium; TOKAMAK is a Russian acronym for a donut shaped chamber). The DT fusion research has made great progress over the past 30 years and the DOE's policy is to keep up the momentum by not allowing diversion of any funds from it to the competing ideas, even if these newer ideas are better. The concept of a Tokamak-based 'crash' program does not originate in USA but in USSR and dates back to 1973. However, the Soviet government killed its Tokamak program in 1975 and we started ours unbeknowingly in 1976, in a belief that it will be done in collaboration with the Russians. Unfortunately, the DT fusion is burdened, to a varying degree, with all the problems of the fission reactors: radioactive waste, weapon proliferaction potential, large size, large shielding, large heat pollution and enormous cost.

It has been said that discovery of an important need was almost as great a contribution as the invention itself. Whoever examines the scientific causes underlying the environmental, political and economic problems of nuclear energy, both fission and fusion, would undoubtedly conclude that what the society needs is nuclear energy without neutrons. We will refer to it as 'aneutronic' nuclear energy.

If one could design an aneutronic nuclear power reactor, such a reactor could never harm a human. If there is aneutronic nuclear fuel, it can never be converted into nuclear weapons. Neutrons, essential for nuclear weapons, are atomic-age villains in civilian applications of nuclear energy. They convert natural uranium into the atomic weapon fuel, plutonium. They can penetrate up to 16 feet of steel and cause structural damage in the toughest of materials, make them radioactive, brittle and accident prone. Present day reactors must be enclosed by thick, concrete shielding, many times the weight of the reactor itself; thus, conventional fission plants are large, uneconomical and therefore unsuitable for decentralized use. They also would not be feasible for mobile or airborne use.

Without neutrons, nuclear explosion or even a 'runaway' accident are not possible; massive shielding is unnecessary; the ability to breed and proliferate nuclear weapons does not exist; there is a potential to produce electricity directly, without heat; and a small, compact lightweight reactor system becomes conceivable.

The existence of aneutronic nuclear reactions, has been known for 52 years, but, mainly because of the lack of miliatry interest, no major effort was made to overcome technical problems which stood in the way of utilizing them as power sources. These reactions are initiated by protons and it was believed that there could be no proton chain reaction-thus, no bomb. The first aneutronic reaction was discovered by Cockroft and Walton in 1932 in Cambridge, England. They bombarded the lithium nucleus with protons and split it into two helium nuclei with a release of a large amount of energy. The energy released per fuel weight was twice as much as that released in the fission of uranium.

The aneutronic reactions can be described as the "splitting of light nuclei by light nuclei", or simply "light fission". The metals used are lithium, beryllium and boron (mined as 'borax'). The process is different from either fission or DT fusion, but since they have more similarities with fusion, they have been called (wrongly) advanced-fuel fasion.

In 1966, Rand McNally has shown that proton chain reactions are possible.

United Sciences Inc., was founded in 1972 (as Fusion Energy Corp.,) to pursue aneutronic research. To date $13 million of private funds have been spent.

The research program is being carried out at the Aneutronic Energy laboratory of United Sciences at Princeton, NJ, in association with a research consortium consisting of Polytechnic Institute of New York, University of Buenos Aires and the University of Graz, Austria. Four laboratory models of the aneutronic reactor named "migma cell" have been built and four stages of a five-stage research and demonstration program had been completed. See Diagram of progress (page 5).

The next--and the last--research stage, Stage 5, will bring migma into the reactor regime i.e. reach or exceed the energy breakeven conditions. It is a 4 year $30 million program. However, if we could use the newly built prototype RFQ accelerator at Los Alamos, it would become a $10 million 2-year program. Prior to embarking into the last stage experiment, we need a one to two-year teoretical and experimental feasibility study program which would cost $1.5 - $2 million/year.

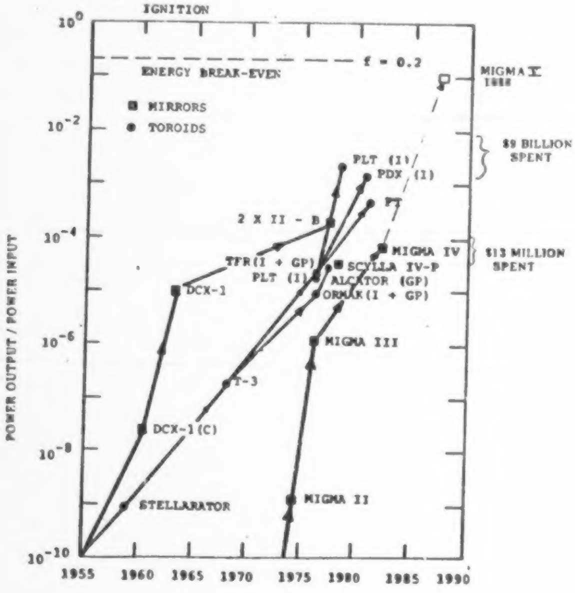
We respectfully request the subcommittee to add-on $1.5 million for aneutronic energy theoretical and experimental feasibility studies to the DOE appropriation for FY 1985.

Summing up, the aneutronic nuclear processes release more energy per fuel weight than nuclear fission, yet they are:

- non-radioactive
- accident-proof
- proliferation-proof -- cannot breed nuclear weapons
- much reduced source of heat rejection to the environment, compared with oil, coal or conventional nuclear plants -- because of direct conversion of energy
- suitable for small size power plants (mass produced units could be stacked into any size plant)
- projected to have low capital and fuel cost

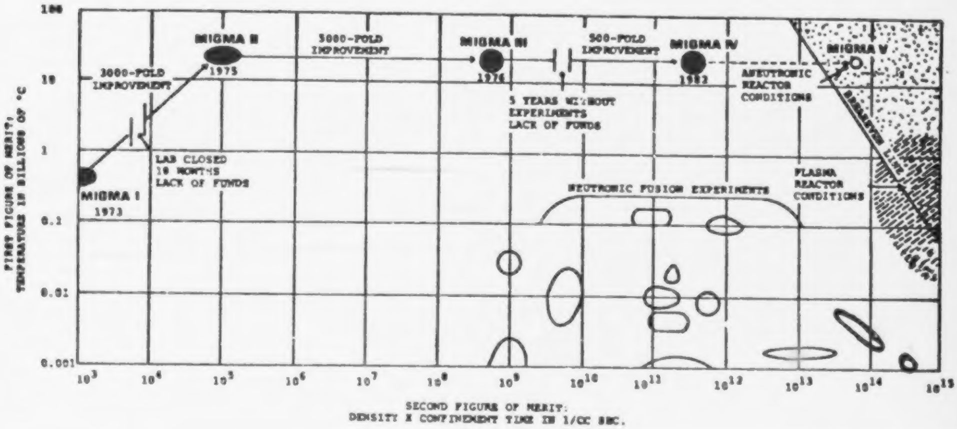
Since radioactivity, runaway-accidents, waste disposal, weapon proliferation potential, heat pollution, large plant size, high capital and fuel cost have been the main areas of controversy for nuclear fission plants being used today, as well as the potential nuclear fusion plants of the current governmental research programs, aneutronic energy offers a radically alternative form of nuclear power.

The U.S. utilites find DT fusion unacceptable for power plants and are against the early construction of DT fusion reactors. States E.L. Draper of Gulf States Utilities: "Their construction will discourage development of advanced fuel cycle (aneutronic) fusion devices which offer more promise for widespread practical application". (Bull. Am. Phys. Svc. Washington Meet. 1984, p.711).



PROGRESS OF DT FUSION VS. ANEUTRONIC

Comparative Progress of Migma (Aneutronic) Experiments (1973-82)
with Current Status of Neutronic Fusion Experiments



There are two generally accepted 'criteria of merit' used to measure progress toward proof-of-principle in nuclear fusion experiments, both neutronic and aneutronic.

The horizontal axis shows ion confinement densities (how compressed the fuel is) multiplied by time of confinement (how long the compressed fuel can be held together).

The vertical axis shows temperature in degrees Celsius. In the case of neutronic fusion experiments, the temperature is typically hundreds of millions of degrees Celsius, but in the case of Migma (aneutronic) research, the typical temperature is two orders of magnitude higher (tens of billions of degrees Celsius).

While Migma achieved its needed temperature goal equivalent to the Migma II experiment in 1975 (30 billion degrees Celsius equivalent), its further goals had still to be reached on the horizontal axis (density x confinement time) and the progress to date is noted.

Net energy production is projected for conditions to the right of the Breakeven Line.

The Migma V experiment - the last stage - is to increase the product another 100 times. This would put migma into the energy producing regime, thus demonstrating scientific feasibility of aneutronic nuclear energy.

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SCHOOL OF ENGINEERING AND APPLIED SCIENCE
LOS ANGELES, CALIFORNIA 90024

March 20, 1984

Congresswoman Marilyn Lloyd
Chairman, Subcommittee on Energy
Research & Production
U.S. House of Representatives
Committee on Science and Technology
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Congresswoman Lloyd:

Being on sabbatical leave from the University of California from January to March, I was out of the country when your letter arrived inviting me to testify on February 29 at the hearing before your subcommittee. The question you addressed, namely, "What activity should be initiated to support the Tokamak Fusion Core Experiment and what specific funding should be provided in FY85," is critical for the Fusion Program. I regret, having just returned to UCLA, that I was unable to testify. Since I am a member of your Advisory Committee and since they have already submitted their findings to you I would add only one general comment.

Right now the U.S., Europe and Japan have become approximately equal with respect to Tokamak fusion research. On the other hand, the U.S. will be the first to study deuterium-tritium plasmas, albeit without ignition. The U.S. is now ready, given what we have put in place, to initiate a true long-pulse, ignition experiment to definitively establish the reactor plasma physics of a Tokamak. Beyond this lies fusion engineering developments to convert the energy from the core to practical power. The Europeans and Japanese are unlikely to commit to this step now because they have just come apace of the U.S. If we are unable to begin now, we will forfeit a great opportunity, forfeit potential U.S. leadership, and not undertake a high technology initiative waiting for us. In several years time our potential competitors will be ready to step beyond us.

We can maintain our leadership with the TFCX initiative and put ourselves in the best position to discuss questions like international cooperation. Should a future opportunity arise, I would welcome it as a means of expressing these and other views to the committee.

Sincerely,

Robert W. Conn
Member, Advisory Committee on
Energy Research & Production

cc: Dr. Jack Dugan, Subcommittee Staff Director
Debbie Martin, Subcommittee Staff Member
Mr. Ron Williams, Subcommitee Staff Member

APPENDIX II

ADDITIONAL QUESTIONS AND ANSWERS SUBMITTED FOR THE RECORD

Department of Energy
Washington, D.C. 20585

March 23, 1984

Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy Research
and Production
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Madam Chairman:

On February 23, 1984, Dr. Alvin T. Trivelpiece, Director of the Office of Energy Research, appeared before your Subcommittee to discuss the FY 1985 authorization for magnetic fusion.

Following that hearing, you submitted written questions for response to supplement the record. Enclosed are answers to those questions.

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

Thomas H. Pretorius
for
Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

cc: Ms. Nancy Jeffery

Questions and Answers

Relating To The

February 23, 1984

Hearing Before The

Subcommittee on Energy Research and Production

Committee on Science and Technology

U.S. House of Representatives

Witness: Alvin Trivelpiece

ADDITIONAL QUESTIONS FOR DR. TRIVELPIECE

Question: The TFCX represents both a scientific and engineering challenge. Does DOE have a position on a configuration for TFCX and, in particular, would that configuration facilitate future upgrades in an effort to minimize the total capital cost of the next two devices?

Answer: The Department of Energy is examining several configurations for a TFCX-like device and has not yet settled on a particular choice. These configurations range from one with minimum initial capital cost to ones with upgrade capabilities and greater initial capital costs. The Department does not yet have a specific opinion as to whether upgrade capability is desirable or undersirable. The selection of the machine configuration will rest on costs and programmatic issues as developed in the current analysis design. Upgrade capability and budgets will be important factors in any selection decision.

Question: While some work is being done on TFCX, it appears that the overall organization to support the TFCX project may not, as yet, be complete. Would you comment on the organization and particularly on the way DOE plans to distribute the work in a TFCX project office so as to include a proper mix and balance of talent and experience for a successful venture?

Answer: A project planning organization has been established under the leadership of Princeton Plasma Physics Laboratory (PPPL) for the planning and conceptual design activities on TFCX. This organization and the working relationships between PPPL and the Department of Energy and between PPPL and the other TFCX planning participants have been formalized and defined in a Management Plan for the TFCX planning and conceptual design. Three key features of the Planning are: (1) It incorporates an integrated organization structure at PPPL which includes key individuals from other contractors holding line positions responsible for device design areas, (2) There is significant involvement of other fusion contractors (ORNL, MIT, ANL, INEL, LLNL) and the Fusion Engineering Design Center in the execution of specific option evaluation and conceptual design tasks, and (3) Three industrial contractor teams will be selected to perform the conceptual design and cost estimation of the TFCX systems. TFCX planning is a national fusion program endeavor and the arrangements that have been implemented provide optimum input from U.S. fusion laboratories, as well as valuable contributions from industry.

Question: How can the Fusion Engineering Design Center (FEDC) in Oak Ridge be used most effectively on TFCX as well as other elements of the fusion program?

Answer: The FEDC is a major part of the design team that is led by PPPL to do a conceptual design for a next step fusion machine. The Design Center provides engineering support for tradeoff studies to select machine parameters, system studies, and preliminary cost analysis of each option. After selection of a machine option, the Design Center will provide detailed design of certain subsystems and plasma engineering direction to the conceptual design work.

A major role of the Design Center is to compare fusion confinement concepts through analysis of power reactor designs. Each reactor design is subjected to the same engineering procedures so that the designs are compared on a common basis. The comparison of tokamak and tandem mirror concepts is essential to the development of an attractive reactor concept by the fusion program.

Question: How competitive are small compact toroids such as ZT-H, and what do you see in the future for their development? How much funding is being directed toward compact toroids?

Answer: Compact toroids (CTs), such as the spheromak and field reversed configuration (FRC), as well as the reversed field pinch (RFP) configuration, of which ZT-H is a proposed device, are at an early stage of development. The present data base for these devices is insufficient to provide an assessment of their attractiveness as a fusion reactor as compared with the larger devices. Reactors based on CTs or RFPs may provide advantages in cost, safety, and design simplicity which, in the future, might make them competitive with the tokamak and mirror machines. Experimental experience with plasma in these magnetic configurations, along with ractor studies, is essential for evolution of the potential advantages these devices may offer. The FY-84 funding for experiments is around $13 million for CTs and $8 million for RFPs.

Question: What is the extent of DOE's effort in developing RF heating, and is such work being properly funded?

Answer: Aggressive experiments leading to an understanding of the physics of radio frequency (RF) heating of fusion plasmas are planned or under way. Technology development of components needed to perform the physics experiments is also under way. Reactor design studies are looking carefully at the ultimate reactor application to provide guidance to the program on the most favorable RF heating approaches to pursue. The funding level for RF heating is balanced within the context of other needed plasma heating development activities. The application of RF heating to large fusion experiments is paced by the ability to effectively utilize the heating systems presently installed and the need to obtain high priority physics results.

DON FUQUA (Fla.), Chairman

ROBERT A. ROE, N.J.
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RICHARD L. OTTINGER, N.Y.
TOM HARKIN, IOWA
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DAN GLICKMAN, KANS.
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ROBERT A. YOUNG, MO.
HAROLD L. VOLKMER, MO.
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FREDERICK C. BOUCHER, VA.

U.S. HOUSE OF REPRESENTATIVES
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Minority Staff Director

March 19, 1984

Dr. Alvin T. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

Thank you for your excellent testimony at our February 23, 1984 Subcommittee hearing on the Department of Energy's budget request for Fiscal Year 1985.

We would like to supplement the record of the hearing with your responses to the attached questions. We would appreciate receiving your responses within five days from the date of this letter. Please forward this information to the Subcommittee office, Room B-374 Rayburn House Office Building, Washington, D.C. 20515, to the attention of Ms. Debbie Martin.

I appreciate your cooperation in this matter.

Sincerely,

Marilyn Lloyd

MARILYN LLOYD, Chairman
Subcommittee on Energy
Research and Production

ML:Mdm
Attachment

ADDITIONAL QUESTIONS FOR DR. TRIVELPIECE

- The TFCX represents both a scientific and engineering challenge. Does DOE have a position on a configuration for TFCX and, in particular, would that configuration facilitate future upgrade in an effort to minimize the total capital cost of the next 2 devices?
- While some work is being done on TFCX, it appears that the overall organization to support the TFCX project may not, as yet, be complete. Would you comment on the organization and particularly on the way DOE plans to distribute the work in a TFCX project office so as to include a proper mix and balance of talent and experience for a successful venture?
- How can the Fusion Engineering Design Center in Oak Ridge be used most effectively on TFCX as well as other elements of the fusion program?
- How competitive are small compact toroids such as ZTH and what do you see in the future for their development? How much funding is being directed toward compact toroids?
- What is the extent of DOE's effort in developing RF heating and is such work being properly funded?

Princeton University
PLASMA PHYSICS LABORATORY

OFFICE OF THE DIRECTOR
JAMES FORRESTAL CAMPUS
P.O. BOX 451, PRINCETON, N.J. 08544
609-683-3555

11 April 1984

The Honorable Marilyn Lloyd, Chairman
Subcommittee on Energy Research and Production
House Committee on Science and Technology
B-374 Rayburn House Office Building
Washington, DC 20515

Dear Mrs. Lloyd:

I am pleased to forward to you the answers to the questions attached to your letter of 28 March 1984 for inclusion in the record of the hearing held on 29 February 1984.

Please let me know if I can be of further help to you.

Sincerely,

Dr. Harold P. Furth
Director

Encl.
HPF:MLS:mae

cc: N. Wells

Q.: What will the TFCX design concept consist of, that is, how much technical data will it contain? Will the general TFCX scope be outlined by FY 1985?

A.: The preconceptual design report for TFCX will be issued at the end of June 1984. This report will document the results of detailed engineering studies for the four configurations that are under consideration for TFCX. These configurations are:

1.	CUTF	A device having conventional copper toroidal field coils, embodied in a simple configuration.
2.	CUTF-HP*	Same as (1) except designed to use higher strength copper alloys and more advanced configurations.
3.	SCTF	A device having superconducting toroidal field coils with nominal current densities.
4.	SCTF-HP*	Same as (3) except design to handle higher current densities and heat loads.

For the preconceptual design, the level of engineering detail must be sufficient to demonstrate the feasibility of designing each subsystem within its spatial envelope and to establish a good cost estimate. The selection of one of these configurations will establish the overall scope of the project.

* HP-high performance

Q.: To what extent will industry participate in the TFCX design team activities?

A.: Industry will be heavily involved in the development of the TFCX beginning in late FY'84 with the conceptual design and planning phase, and continuing through with the detailed design, construction, installation, and commissioning of the project. Industry will also participate to a significant degree in the operation of the facility when it is completed.

The conceptual design of TFCX will be done over a period of 15-19 months by a joint work force of up to three industrial subcontractor teams. This work is expected to begin late in FY'84 and end early in FY'86. The product will be a detailed conceptual design of TFCX, a cost estimate, and long-range plans for the project.

Pending approval by Congress of TFCX as a project, detailed design can begin as early as FY'86. This design work will be done by a single industrial subcontractor who will be fully responsible for systems integration and detailed design.

Once TFCX is commissioned in the early 1990's, industry participation will continue in the operation of the facility. This is a departure from previous practice in the fusion program. and is based upon the need to have industry assume the dominant role for all phases in the Experimental Test Reactor (ETR) that would be a logical successor to the TFCX.

Q.: Are we placing sufficient emphasis on rf heating, and if not, what additional work could be undertaken?

A.: Rf heating physics and technology development programs are in progress at several operating tokamaks in the United States. For example, the Princeton Large Torus (PLT) program at Princeton is devoted entirely to experiments on rf heating. Rf heating experiments are being given high priority on the ALCATOR device at MIT. Rf heating is being developed in Europe for the JET experiment and in Japan for the experiments on JT-60.

The TFCX design project has listed among its special R&D needs rf physics and technology. A report has been transmitted to OFE and plans are now being developed to address the TFCX R&D needs as part of the base fusion program.

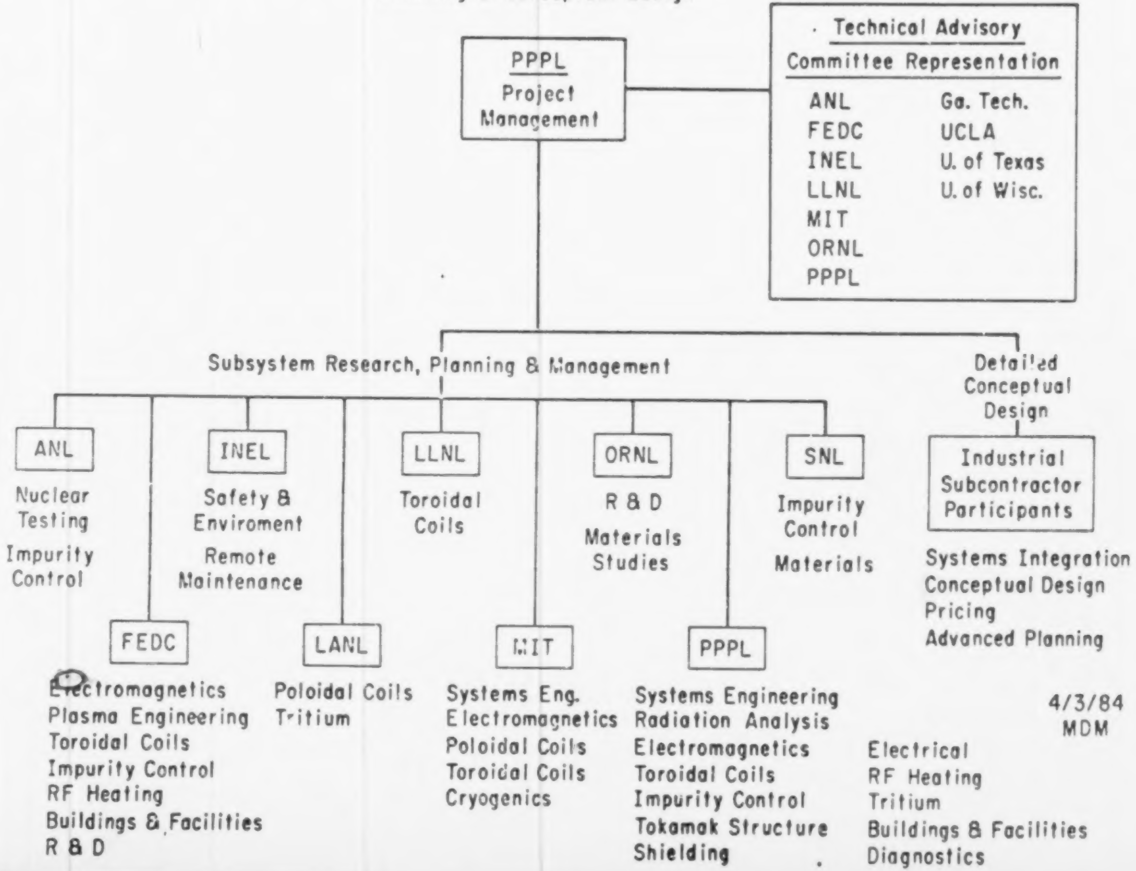
Q.: In your written testimony, you mentioned a national TFCX design team. Can you suggest an organizational chart which describes all the participants in the TFCX team including all those in the national laboratories. industry, universities, etc. who are to be involved?

A.: Please see the attached chart.

#84A0031

TFCX PARTICIPANTS

Planning & Conceptual Design



GA Technologies

GA Technologies Inc.
P.O. BOX 81608
SAN DIEGO, CALIFORNIA 92138
(619) 455-3000

April 11, 1984

U. S. House of Representatives
Committee on Science and Technology
Subcommittee on Energy Research and Production
ATTENTION: Ms. Debbie Martin
Room B-374 Rayburn House Office Building
Washington, D. C. 20515

Gentlemen:

Enclosed are my responses to additional questions transmitted by Marilyn Lloyd in her letter of March 28.

Sincerely,

Tihiro Ohkawa

TO:shk

Enclosures (2)

RESPONSES TO ADDITIONAL QUESTIONS FOR DR. OHKAWA

- In your written testimony, you mentioned that level funding would cause an erosion of the base program. Are there any aspects of the fusion program, or any work, which has been completed that can now be closed out?

  The pace of the program is being determined by the funding level and not by the technical aspects. All elements of the program are productive. If certain projects were to be shut down, it will be a priority issue and not a technical issue.

- Since we have used auxiliary heating and pellet injection, would you describe the specific R&D effort that might be undertaken in direct support of TFCX?

  High power ion cyclotron heating of shaped, high β plasmas in DIII-D.

RESPONSES TO ADDITIONAL QUESTIONS FOR DR. OHKAWA

Minority Questions
Energy Research and Production Subcommittee

- Please describe the OHTE program.

GA Technologies Inc. is developing a new magnetic fusion concept called "OHTE" under the joint research and development agreement between GA Technologies Inc. and Phillips Petroleum Co. OHTE is a doughnut-shaped toroidal magnetic fusion system carrying a current in a hot ionized gas (plasma) much like a tokamak. However, OHTE is a high current density device like conventional pinch systems which have current densities much higher than a tokamak. OHTE differs from a conventional toridal pinch system because of its helical magnetic coils wound around the torus that are expected to improve stability and confinement of a plasma.

Thus OHTE has a potential of developing a compact fusion system which ignites by ohmic heating due to the plasma current alone without additional heating such as neutral beam or radio-frequency wave heating required for tokamaks. This can minimize technology development and lead potentially to a shorter time scale and lower cost for fusion development.

The present joint development program started about three years ago and has produced encouraging results in the small experimental device operating at GA. Very high β-values (ratio of plasma pressure and magnetic pressure) up to 20% have been obtained. The plasma confinement is good enough so that ignition by ohmic heating is possible. Based upon the results, internal discussion of the next step of the OHTE program aimed at ignition is underway between Ga and Phillips.

ARGONNE NATIONAL LABORATORY
9700 South Cass Avenue, Argonne, Illinois 60439
Telephone 312/972-

April 9, 1984

Ms. Debbie Martin
Subcommittee on Energy Research and Production
Committee on Science and Technology
B-374 Rayburn House Office Building
Washington, DC 20515

Dear Ms. Martin:

The following answers are provided in response to the questions contained in Chairman Lloyd's letter of March 28, 1984.

Question 1

- You mentioned work directed toward achieving higher Beta levels and also work which could lead to reductions in magnetic field strength. Is there work now in the program that is specifically aimed at these goals relative to the Tokamak?

Answer

- Yes, there are both experimental and theoretical programs in place to explore higher Beta limits for Tokamaks. The BIG-DEE modification of Doublet-III at GA Technologies in San Diego will explore Beta values up to about 10% in a plasma of a size and shape similar to the proposed TFCX. Also, the PDX device at the Princeton Plasma Physics Laboratory has been modified to explore higher Beta values in so-called "bean" shaped plasmas. The ATF device under construction at the Oak Ridge National Laboratory will study higher Beta values in stellarator devices. There are theoretical studies in several fusion laboratories and universities. Finally, Argonne National Laboratory plans to carry out a Tokamak reactor study in FY-1985/1986 which will update STARFIRE and explore the reactor implications of higher Beta values for Tokamaks. Higher Betas imply the ability to decrease the magnetic field strength with the same total power output.

Question 2

- In your written testimony, you made the statement, "....the selection of the first wall and blanket can significantly impact plasma physics and engineering issues including the selection of suitable confinement concepts." If the decision were yours alone, would you design TFCX to include a tritium-producing blanket or would you introduce ways to upgrade TFCX at a later date to include a full blanket.

Answer

- The TFCX's primary goal is to accomplish long-pulse ignited fusion burns for several minutes. The present preconceptual designs for TFCX do not include high duty cycle operation or extended operation with deuterium and tritium. (The primary reason for these limitations is to hold down the cost.) Such a device is not very useful for blanket tests. I would prefer a more ambitious step to include the ability to test blankets, but this can only be accomplished with increased funding for TFCX and the overall national program. (Even if a blanket is not included in the initial operation of TFCX but is added later in an upgrade, it will be necessary to make provisions in the design from the very beginning of the project.) Such a step would take us much closer to demonstrating the engineering feasibility of fusion. It must be noted though that it would be necessary to significantly expand the present base technology programs in blanket and materials R&D so that we could be prepared to put a blanket on TFCX or its upgrade.

Question 3

- You referred to upgrading "test stands." Do you mean devices like PLT, and if so, which devices could offer the most useful information if upgraded?

Answer

- I was referring to upgrades or construction of new hardware for various types of first wall/blanket/materials test stands, not plasma confinement devices. We can and must learn a great deal about the development of such components from non-plasma test stands. Examples include the following:

  - High heat flux testing with electron beam facilities at Westinghouse (called ASURF) and an ion beam facility at Sandia/Albuquerque (called PMTF). The ASURF facility should be upgraded to higher power levels.
  - Study of eddy current and electromagnetic effects in the Fusion Electromagnetic Induction Experiment (FELIX) which has recently been constructed at Argonne National Laboratory. This device should be upgraded to higher magnetic field values.
  - Upgrade of liquid metal corrosion and MHD loops at Argonne National Laboratory to study key issues of lithium and lithium-lead blankets.
  - Further inpile fission reactor tests of tritium extraction and recovery from solid lithium ceramic blankets.
  - Construction of a test stand to study the heat extraction issues from solid lithium ceramic breeder blankets.

- Modifications of the Tritium Systems Test Assembly (TSTA) at Los Alamos National Laboratory to include the processing of tritium streams from blanket systems.

- Upgrade of the RTNS-II at Lawrence Livermore National Laboratory for further 14 MeV neutron studies.

Minority Questions

- Your strong support for the TFCX is noted. What contributions to this program do you anticipate for the Argonne National Laboratory?

Answer

- Argonne National Laboratory is participating in the TFCX preconceptual design in areas of impurity control engineering, materials research and nuclear testing. We anticipate future support in these same areas as well as support in tasks to resolve electromagnetic design problems, testing of impurity control components, nuclear analysis method development, and development of pulsed superconducting magnets.

Please let me know if I can be of further assistance to the Subcommittee.

Sincerely,

Charles C. Baker

Charles C. Baker
Director
Fusion Power Program

CCB:rm

Lawrence Livermore National Laboratory

April 12, 1984

Ms. Marilyn Lloyd, Chairman
c/o Ms. Debbie Martin
Subcommittee on Energy
Research and Production
U.S. House of Representatives
Committee on Science and Technology
Room B-374, Rayburn House Office Building
Washington, D.C. 20515

Dear Ms. Lloyd:

We are pleased to supplement the February 29, 1984 Subcommittee hearing record on the Department of Energy's request for Fiscal 1985 by answering your question:.

"Based on Livermore experience, what work should be pursued to improve RF heating, and does the FY 1985 budget provide for such work?"

Our answer is attached on your enclosed questionnaire.

Sincerely,

Carl D. Henning

Carl D. Henning
Associate Program Leader
for Fusion Development & Technology

CDH:ds
attachment

ADDITIONAL QUESTION FOR DR. HENNING

- Based on Livermore experience, what work should be pursued to improve RF heating, and does the FY 1985 budget provide for such work?

ANSWER:

There are two regimes of RF heating important to the fusion program: High frequency 50-140 GH_3 and low-frequency 6-100 MH_3. The high frequency gyrotron and launcher developments are of interest to nearly all of the fusion concepts, including tokamaks and mirrors. The development of higher power and efficient gyrotron tubes (1 - 2.5 MW) is of particular importance to the mirror program, while quasioptical launching systems can benefit both the tokamaks and mirrors.

Varian Associates and the Naval Research Laboratory have been the principal developers of gyrotrons, while LLNL, the University of Wisconsin, and TRW Incorporated have been working on the quasioptical launchers. These systems can be tested at the ORNL RF facility. All parties have been under funded, with little improvement in FY 1985.

Low frequency systems such as Ion Cyclotron Resonance Heating and Lower Hybrid Current Drive at 6 - 100 MH_3, are quite different. Here the power systems tubes and waveguides are reasonably developed and previously used in large radar facilities. It is the antenna design and coupling to the plasma that are important. Some preliminary work can be done on feedthroughs, in particular, at the ORNL RF facility, but most of the plasma coupling and specific antenna design must be accomplished on existing major plasma confinement devices like TMX-Upgrade, Alcator C, Doublet III and TFTR. Because of the importance of current drive to the tokamak program, an aggressive program is warranted. However, most of the money should be directed to confinement projects for short term antenna development.

MCDONNELL DOUGLAS ASTRONAUTICS COMPANY

ST. LOUIS DIVISION

Box 516, Saint Louis, Missouri 63166

6 April 1984

Ms. Marilyn Lloyd, Chairman
Subcommittee on Energy Research & Production
Committee On Science and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D.C.   20515

Dear Chairman Lloyd:

Enclosed are the answers to the supplementary questions you asked concerning the 29 February 1984 Congressional hearing.

Thank you again for asking the IEEE to testify.

Sincerely,

Allan T. Mense

Allan T. Mense
Senior Scientist

ATM:lmm

Enclosure: (1) Answers to Supplementary Questions

MCDONNELL DOUGLAS
CORPORATION

ANSWERS TO SUPPLEMENTARY QUESTIONS
FROM DR. ALLAN T. MENSE

Question #1

In your written testimony, you mentioned the lack of funding support for the Fusion Program relative to other S&T programs. While I agree with your findings, let me pose a question. Suppose you had to provide for the Fusion Program within the amount budgeted by DOE, what would be your priorities in order of importance?

Answer

Since detailed priorities cannot be determined without specific information available to only the Department of Energy, only a very general prioritization can be made. One reasonable such listing would be:

TFTR Operation and Support
Doublet III - Big D Construction and Support
TFCX Design
MFTF-B Construction
Stable University Research Funding Support
Fusion Materials Development and Testing

There are obviously very important smaller programs which are deserving of high priority funding. I have attempted a listing of major research areas within the MFE program and have shown what I see as the top six areas.

Question #2

How mature would you say fusion is, in terms of producing energy? Specifically, do you see any problems in engineering that need to be resolved in the near future, or should we continue our scientific push first with a more modest TFCX without a blanket and larger coils? Wouldn't this enhance our probability of achieving a successful "long burn" after ignition?

Answer

Magnetic Fusion is the least mature of those "energy" technologies which can actually produce net power output. It does, however, have the highest of long term potentials and will undoubtedly produce the largest number of high technology spin-offs during its course of development. There are no insurmountable engineering problems which must be solved prior to designing TFCX. The ultimate long term performance of TFCX may, however, depend critically on the material performance characteristics of the electrical insulation and/or other material related components used in the device. The number of additional reactor relevant technologies which TFCX could address would be directly proportional to how much funding above the $1 billion level the government is willing to provide. The TFCX mission is budget driven, not technology driven. With a design-to-cost objective of $1 billion, the only major mission which the TFCX can address is ignition and "long pulse" burn. The probability of its success is probably less, not greater, with the above mentioned spending cap.

ANSWERS TO SUPPLEMENTARY QUESTIONS (CONT.)

FROM DR. ALLAN T. MENSE

Question #3

In its endorsement for the TFCX program, what extent of participation does the IEEE anticipate for industry?

Answer

If the TFCX program is carried out within the envelope of a completely level budget, there will be no increase in overall industrial participation. This presumes that participation is measured by the total dollars spent in industry by the Department of Energy fusion laboratories, mostly to fabricate components and purchase components. However, industry will have a more responsible role in the overall fusion power program if the TFCX project is initiated and carried through to fruition. This new type of major management and systems integrator role is very important if "industry" is ever to really understand fusion well enough to make an eventual decision on how and when to commercialize it.

EBASCO SERVICES INCORPORATED **EBASCO**
Two World Trade Center, New York, N.Y. 10048

Leonard F. C. Reichle
Group Vice President
Advanced Technology
and Special Projects

April 11, 1984

The Honorable Marilyn Lloyd, Chairwoman
Subcommittee on Energy Research
& Production
U S House of Representatives
Committee on Science & Technology
Suite 2334, Rayburn House Office Bldg.
Washington, D. C. 20515

Dear Congresswoman Lloyd:

This acknowledges receipt of your letter of March 28, 1984 attaching questions on which you request my responses for supplementing the hearing record at which I testified before your Subcommittee on February 29.

My responses to your questions are attached hereto. I appreciate the opportunity to supplement my testimony.

Best wishes!

Sincerely,

Len

LFCR:ia
Enclosure

Responses by Mr. Leonard F C Reichle to Additional Questions Relating to His Testimony on 2/29/84 Before the Subcommittee on Research & Production of the House Committee on Science & Technology

1 - How Can An Industrial Team Be Held Responsible for the Design of TFCX?

The Industrial Team can be held responsible for the design of the TFCX if the U. S. Department of Energy (DOE): (a) requires the Princeton Plasma Physics Laboratory (PPPL) to define and issue to the competing Industrial Teams the design objectives and the parameters within which a TFCX conceptual design will be acceptable, and advises each Team that it is free to develop any conceptual design it believes is best to achieve the design objectives within the established parameters, (b) provide through PPPL to each Industrial Team equal and adequate funding to perform a design, (c) make available from DOE-funded programs at PPPL, the ORNL Fusion Design Center, etc. codes, computer time, and a reasonable amount of technical staff time to assist each Industrial Team in the development of its conceptual design, and (d) requires PPPL to notify each of the competing Industrial Teams that the Team producing the best conceptual design will be selected for project management, design engineering, procurement, installation and assembly of the TFCX if it is authorized to proceed as a Project.

In this approach, each Industrial Team will feel and, in fact, will be fully responsible for the conceptual design evolved by it. Moreover, each Team will be motivated by the incentive that, if it produces the winning conceptual design, it will have the opportunity to design and build the real TFCX. The DOE through PPPL will control the TFCX Program. The Industrial Teams will assign their best people, participate in funding, and give the TFCX Project top management visibility and attention.

The responsibility of the Industrial Contractor (and his Team Subcontractors) selected for the TFCX Project will be comparable to the responsibility shouldered by any cost-type industrial contractor performing systems integration, R&D, design engineering, procurement, installation and assembly of a developmental item for the nation's Nuclear Program, Defense Program or Space Program. The Industrial Contractor and his Team Subcontractors will perform responsibly because their corporate reputations and their futures in the fusion power business are at stake. Moreover, the TFCX contracts entered into undoubtedly will contain material and component quality standards; services performance standards for engineering, fabrication and installation; re-work provisions; liability limitations; etc.

2 - If an Industrial Team carried the TFCX through design and equipment procurement to checkout and test, what participation would you expect from the laboratories and universities?

The university or laboratory would be the prime contractor under DOE (as it was for TFTR) and would enter into contracts with the lead company of each conceptual design team and, after the winner is selected, enter into a contract with the lead company of the winning team to perform the role of Industrial Subcontractor to the university or laboratory (as Ebasco was Industrial Subcontractor to PPPL for TFTR). Thus, the university or laboratory would have over-all programmatic and financial cognizance over work performed by the Industrial Subcontractor.

Universities and laboratories of the fusion community would participate by providing: physics knowledge and advice, design information, computer codes and computer operating time, experimental fusion data and experience, and other guidance and assistance as required.

3 - Your testimony makes a very strong case for industry involvement in the TFCX program. What reception from the fusion community does such a request receive?

Reception of strong industry involvement varies. Some look upon it as something "premature, i. e. better reserved for the time when fusion is closer to commercialization" or as something that may "diminish the importance of the laboratories and universities or force them to share their budgets with industry." Others welcome it as the best way of assuring realistic and cost effective development of the nation's fusion program.

Actually, strong industry involvement in fusion as it proceeds further into the engineering phase of development should not be looked upon as threatening the laboratories and universities. Rather, it should be looked upon as a continuation into the TFCX program of the kind of the strong industrial role performed in the TFTR Program by Ebasco as PPPL's Industrial Subcontractor, with Grumman as Ebasco's

Team-Subcontractor. Strong industrial involvement in the TFCX Program will provide the opportunity for Government, universities, laboratories and industry to form a partnership (with each component of this partnership performing what it can do best) which will enable the United States to compete most effectively in fusion energy development with the Japanese, Western Europeans, and the Soviet Union.

Columbia University in the City of New York | *New York, N.Y. 10027*

SCHOOL OF ENGINEERING & APPLIED SCIENCE
Office of the Dean

4 April 1984

Marilyn Lloyd, Chairman
Subcommittee on Energy
Research and Production
Room B-374
Rayburn House Office Building
Washington, D. C., 20515

Dear Madam Chairwoman:

This letter is written in response to your letter of 28 March 1984 which contained additional questions concerning the Magnetic Fusion Energy Program.

1. What activities in the base program have been successfully completed?

Reply: In research programs there is always more that can and perhaps should be done. Which base programs have been successfully completed can best be determined by the DOE Office of Fusion Energy Staff, perhaps using the advice of an ad hoc panel.

2. In the report... do the universities have working agreements with the National Laboratories and industry where the universities could assign students to base program devices on a temporary basis?

Reply: Some but certainly not all universities with plasma-fusion programs have close working relations with National Labs and/or industry. By assigning students to work at a National Lab (or industry), a different kind of educational experience is achieved. For some students, and for some research problems, this is clearly the right way to go. At Columbia University we have at different times sent students to Sandia, Los Alamos, and Argonne National Labs. It was nearly always because the National Lab had unique equipment or unique experience which was needed for research we wanted to do. Of the approximately 80 Ph.D.'s in plasma physics granted by Columbia, perhaps five did the majority of their research "off campus" at a National Lab. The vast majority did their research "on campus" where they could build their own experiment, take risks and make mistakes that are usually not acceptable in a larger National Lab program. The educational experience is, frankly, just different. Each (on or off campus) has its merits and problems. Most faculty definitely want "on campus" research programs. For research universities it is vital.

3. Do you know of any existing fusion device which has outlived its usefulness and could be relocated to a university?

Reply: If a fusion device has "outlived its usefulness" then a good university would not want it and DOE should not fund its operation no matter where it is located. If a device can be modified at modest cost/effort, then its relocation and cost of operation should be judged on the merit of the proposed research.

4. The MFAC report on the role of universities mentions the contributions made by the universities. Do you believe that university teams can play a role in compact Tokamak research?

Reply: University teams definitely can and should play a role in compact Tokamak (and all compact fusion concepts) research. When faculty have interesting ideas which can lead to smaller and more compact fusion reactors, the basic reasearch and testing of these ideas often is best done at the universities, with the faculty and students who created the concept.

Sincerely,

Robert A. Gross

Robert A. Gross
Dean

RAG:mp

ADDITIONAL QUESTIONS FOR DR. GROSS

- What activities in the base program have been successfully completed?

- In the report attached to your testimony concerning the role of universities in the fusion program, the building of a number of intermediate fusion devices or experiments costing up to $30 million each was mentioned. In order to provide an alternative to train students, do the universities have working agreements with the National Laboratories and industry where the universities could assign students to base program devices on a temporary basis?

- Do you know of any existing fusion device which has outlived its usefulness and could be relocated to a university?

- The MFAC report on the role of universities mentions the contributions made by the universities. Do you believe that university teams can play a role in compact Tokamak research?

FUSION POWER ASSOCIATES
2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879
(301) 258-0545

STEPHEN O. DEAN
PRESIDENT

April 12, 1984

Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy
Research and Production
Committee on Science and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Bldg.
Washington, D. C. 20515

Dear Mrs. Lloyd:

Thank you for your letter of March 29 requesting my response to the following questions.

Question 1: What is your opinion of the need for going ahead with FMIT considering the two rotating neutron sources at Livermore and other material irradiation research? Would not FMIT provide a fluence of 10 times the fusion rate or equal to the RTNS at 1013?

Response: The FMIT is a necessary facility which is intermediate in capability between our present neutron sources at Livermore and a real fusion reactor materials test facility, which as you know, is not presently going to be funded in the near future. If we were building a fusion engineering device (FED) at this time, perhaps we could by-pass the FMIT. But in its absence, it seems to me that we will require FMIT to provide data for an ambitious fusion engineering device to be committed in the 1990's.

Question 2: Does DOE need to do a study to determine what engineering facilities are needed?

Response: DOE presently has a study to identify the engineering facilities needed. This study is in progress at UCLA. A report of work in progress was made at my recent annual meeting in Livermore and I commend the results of that study to you as it nears completion.

"TO TAKE FROM THE SEA AND TO IMITATE THE SUN"

Question 3: Your testimony summarizes the Energy Research Advisory Board review of DOE's Comprehensive Program Management Plan. How do you believe that DOE will respond to the ERAB critique?

Response: As far as I am aware at this time, DOE does not intend to revise or update its recently submitted Comprehensive Program Management Plan even though the strategies presented in that plan are not consistent either with the ERAB report, the MFAC advice or even DOE's actual management initiatives for TFCX.

Thank you again for the opportunity to testify to your committee.

Sincerely,

Stephen O. Dean

Stephen O. Dean

MCDONNELL DOUGLAS ASTRONAUTICS COMPANY
ST. LOUIS DIVISION
Box 516, Saint Louis, Missouri 63166

30 May 1984

Ms. Debbie Martin
Subcommittee Office, Energy Research and Production
Room B-374 Rayburn House Office Building
Washington, D.C. 20515

Dear Ms. Martin:

Mr. Harold Motchan gave fusion energy testimony at the February 29, 1984, Subcommittee hearing on the Department of Energy's budget request. Subsequently, additional questions were forwarded to Mr. Motchan by the Chairman. Response to these questions are attached.

Mr. Motchan has been on sick leave which contributed to the delay.

Very truly yours,

D. A. DeFreece
Program Manager, Fusion Energy
Dept. E290, Bldg. 278

DAD:LB

MCDONNELL DOUGLAS
CORPORATION

Mr. Harold Motchan:

- In your testimony you state that, "the next device must have a Plant Capital Equipment cost less than $1 billion." Do you refer to the Tokamac Fusion Core Experiment (TFCX)? What degree of probability do you assign such an occurrence? What all will be required to meet such a goal?

The comment refers to TFCX. I believe the cost goal is achievable if the project adopts the approach that TFCX is a minimum step for real progress. This will require well defined objectives and extensive scoping studies of how best to meet the objectives at lowest cost. These studies are currently in progress. Further, multiple detailed conceptual designs, including costs and technical risks, should be completed prior to selection of the final configuration. The selection should be made on the basis of meeting the objectives at lowest cost and cost risk. However, designing the device to allow upgrading on the basis of new experimental data should be considered in selecting the final configuration. Finally the project will require strong leadership to complete the job on cost and schedule.

- Excluding the drafting of conceptual drawings, what engineering work would you feel should be undertaken in FY 1985 that is not currently scheduled?

Engineering work on the TFCX must achieve sufficient depth in the conceptual design phase to confidently select the configuration for Preliminary design. Confidence comes from designing and costing competitive concepts in sufficient detail to reduce technical and cost uncertainties. Sufficient depth could be achieved through an approximately $20 million conceptual design effort; divided approximately equally between national laboratories and industry to make the best mix of their different experience.

- In your written testimony, you mentioned "a change in priority away from the widespread base program." Can you identify specific technical areas which may have matured to completion?

One can seldom say that a technical area has matured to completion because there is always more that can be learned. Generic technology study, however, is seldom as productive as project driven R&D which must produce knowledge essential to the success of the project. Giving priority to work that is needed for a new TFCX subsystem, for example, will advance technology most efficiently.

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* NOT ADMITTED IN D.C.

April 16, 1984

The Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy
Research and Production
U.S. House of Representatives
Committee on Science and
Technology
Suite 2321 Rayburn
Washington, D.C. 20515

Dear Congresswoman Lloyd:

Pursuant to your request, I have enclosed the Answers of Dr. Ramy Shanny to Questions of the Majority and Minority Staff relating to Dr. Shanny's Testimony at the February 29, 1984 Subcommittee hearing on the Department of Energy's budget request for Fiscal Year 1985.

Sincerely,

Gary J. Klein

Gary J. Klein

GJK:vki
Enclosure

RESPONSE TO ADDITIONAL QUESTIONS FOR DR. SHANNY

QUESTION: What are the specific reactor conditions that you hope to achieve, and are there any experiments which support your optimism in achieving these conditions?

ANSWER: Plasma ignition and subsequent stable equilibrium burn viz. all density and temperature profiles, helium ash and other impurities have reached their steady-state values with:

(1) Plasma β in the range $2\% \leqslant \beta \leqslant 8\%$.

(2) Density x confinement time products $n\tau \geqslant 3 \times 10^{20}$ sec/m^3.

(3) Fusion power $\div$ plasma heating input power, $Q \rightarrow \infty$.

(4) Average electron and ion temperatures $\bar{T}_e$ and $\bar{T}_i$, respectively, in the range $8 \text{ keV} \leqslant \bar{T}_e, \bar{T}_i \leqslant 15 \text{ keV}$.

(5) Average electron and ion densities $\bar{n}_e$ and $\bar{n}_i$, respectively, in the range $5 \times 10^{20}/\text{m}^3 \leqslant \bar{n}_e, \bar{n}_i \leqslant 2 \times 10^{21}/\text{m}^3$.

(6) Plasma impurity levels (Z_{eff}) in the range $1.2 \leqslant Z_{eff} \leqslant 2$.

The present level of the understanding of tokamak physics, as determined by the DOE funded program through such devices as Alcator A, Alcator C, PLT, PDX, DIII and TFTR, provides the optimism and data upon which we have based our program. Specifically, PLT has just entered our proposed range for T_i, Alcator C operates within our density range, PDX and DIII within our β range. All machines operate within the Z_{eff} range.

The above listed targets are based on solid experimental data. The following targets which INESCO, Inc. hopes to attain for Reactor operations are based on more tentative evidence and theoretical assertions.

It is INESCO's hope that beta values well in excess of 0.1 (10%) will be demonstrated in a stable burning plasma. This target is justified on the following basis. Experimental evidence that Beta limits increase with increasing current. The plasma current in a Riggatron is typically 6-8 MA (nearly an order of magnitude larger than in present experiments). In addition, theoretical studies performed extensively in the DOE sponsored program predict Beta values well in excess of 20%. Possibly a more significant fact would be the excess of energy produced in a burning plasma which more than compensates for the increase in plasma energy losses as the Beta value increases.

In addition to the above, it is hoped that plasma ignition would be attained at toroidal magnetic field values 10%-20% less than current Riggatron/FDX designs. This hope is substantiated by Alcator C's latest results on transport. Should the above occur, a more efficient cooling system for the reactor can be devised allowing for a better energy balance. It is hoped that under these conditions circulating electric power of substantially less than 50% for a pure fusion reactor can be achieved.

QUESTION: Why will the Soviets not be able to achieve a "sustained burn" with their small copper coil tokamaks?

ANSWER: The small copper coil Soviet tokamak (T-14) will not be able to achieve a "sustained burn" as its planned operational pulse length is too short.

The stated main goal of the Soviet program is to achieve thermonuclear conditions ($T \sim 7$ keV, $n\tau \sim 10^{20}/m^3$) and study the plasma behavior at significant fusion energy release ($Q \geq 1$) in a relatively inexpensive experimental device so as to:

(1) provide the information needed for the design of an elaborate ignition and burn experiment, and

(2) evaluate some possible technical solution for the experiment.

Their emphasis has been in the development of a very large (900 MJ) inductive energy storage power supply and in making simple, inexpensive and changeable tokamaks. This type of power supply, while ideal for supplying the rapid toroidal field compression utilized in their tokamaks, does not lend itself to the long pulses needed to demonstrate sustained burn. In addition the pulse length has been chosen short enough to preclude the need to actively cool the magnets, resulting in a simple magnet design. This compromise, however, totally prevents the use of the magnets in the extended pulse mode needed for sustained burn, as they would melt.

In order to construct a compact tokamak device that can operate in a sustained burn mode, the basic requirement is the availability of high strength, high conductivity ductile alloys and advanced fabrication techniques, as well as stress/strain compatible, inorganic insulators, and associated fabrication techniques. This technology exists at present only in the United States, and is based on INESCO's proprietary technology. These developments were possible based on the superior technological and industrial infrastructure which exists in the U.S., but does not exist in the Soviet Union.

RESPONSE TO ADDITIONAL QUESTIONS FOR DR. SHANNY

MINORITY QUESTIONS

QUESTION: At your present rate of development, at what time will the necessary engineering reach the present science levels in your magnetic fusion device?

ANSWER: The materials and fabrication development programs underway, together with the thermal hydraulic critical heat flux measurements planned, should answer all outstanding engineering questions by the end of 1985.

QUESTION: What schedule do you see for the application of your development? What do you believe the most likely application will be?

ANSWER: It is anticipated that INESCO's Fusion Demonstration Experiment (FDX) program will be completed around 1990 with the successful operation of the first family of ignition and burn devices. This will allow the community to ascertain under what conditions it will be possible to construct a commercial fusion reactor. Would it be best to go with the superconducting approach or use resistive magnets; is pure fusion possible, or do hybrid schemes make more commercial sense? Additional possible uses such as synthetic fuel production, industrial process heat and nuclear plant fuels production would be evaluated at that time.

Until fusion ignition and burn have been fully characterized, possible applications cannot be accurately identified.

QUESTION: You suggest that DOE and INESCO work closely together. With the proprietary data base which has been established by INESCO, what technology transfer do you see as possible?

ANSWER: On a confidential and protected basis, INESCO is prepared to share with the DOE all of its technology and proprietary data base relevant to any jointly conducted cost-shared program. Safeguards will have to be devised which will protect INESCO's commercial interests.

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April 11, 1984

Marilyn Lloyd
Suite 2321 Rayburn House Office Building
Washington, DC 20515

Dear Cong. Lloyd,

Thank you again for the opportunity to testify before your subcommittee on the fusion program.

As you requested, I have fowarded responses to the minority questions to Ms. Debbie Martin, and have enclosed a copy for your attention.

Please let me know if there is any other way in which we could be of service to the subcommittee. For the Fusion Energy Foundation,

Sincerely,

Marsha Freeman

Publishers of Fusion Magazine, the International Journal of Fusion Energy, and The Young Scientist

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Answers to Minority Questions
Magnetic Fusion Hearing
February 29, 1984

* The reference in my testimony to developing policies based on the notion that "science and technology programs are a luxury" was a specific reference to Science Advisor George Keyworth's frequent statement that the United States can no longer afford to be number one in various scientific fields. It is as though these programs were not vitally needed for the economic and military strength of our nation.

Anti-growth organizations, such as the Club of Rome, have tried to make the case that the world faces zero growth and that science cannot do anything to change that. They overlook the fact that throughout historical ages, new technologies made possible through breakthroughs in science, have created new resources allowing the human species to grow and develop.

Organizations such as the Heritage Foundation, which would have the U.S. throw away its lead in science and technoloug fields because of some notion that the "market place" will invest in this R&D, have thereby also made this research a "luxury" rather than a national necessity.

Anti-nuclear groups, including those funded by the Ford and Rockefeller Foundations, have tried to stop the advance of nuclear technoloyg using various spurious arguments. Here, too, advancements in energy and other technologies are seen as a "luxury" which the U.S. can afford to do without. Unfortunately, other nations in the world are not as bestowed with natural resources as the U.S., and in these nations people are dying due to a lack of food, fresh water, energy and medical care, all of which depend upon electric power, which nuclear power could provide.

Studies done by the Fusion Energy Foundation have shown that it is precisely the rate at which new technology is introduced into a society that determines whether and how fast it will grow. Science and technology research programs are a national commitment and national investment, which should not be held hostage to annual budget problems or the whims of special interest groups.

* The Fusion Energy Foundation generally supports the recommendations made by the Energy Research Advisory Board, in terms of the need for a next-step device in the magnetic fusion program. It has been agreed upon by the fusion scientists that this next step would be an ignition experiment.

If, however, the ERAB's funding recommendations were followed, it would mean other still-productive fusion experiments would have to be delayed, stretched out, or shut down.

In their report, the ERAB states, "Under present budget restraints, it appears feasible to pursue this course within the present budget levels for several years." If this is done, the rest of the program will continue to lose talented people and momentum.

We would rather support the Magnetic Fusion Advisory Committee's (MFAC) recommendations, which recognize the need to increase the budget to accommodate this new machine. In reality, the magnetic fusion program today is "funding limited" and many worthwhile ideas are not being pursued. The Fusion Energy Foundation would propose that the program instead be "technology limited" while it moves ahead as quickly as possible.

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